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25 April 1958

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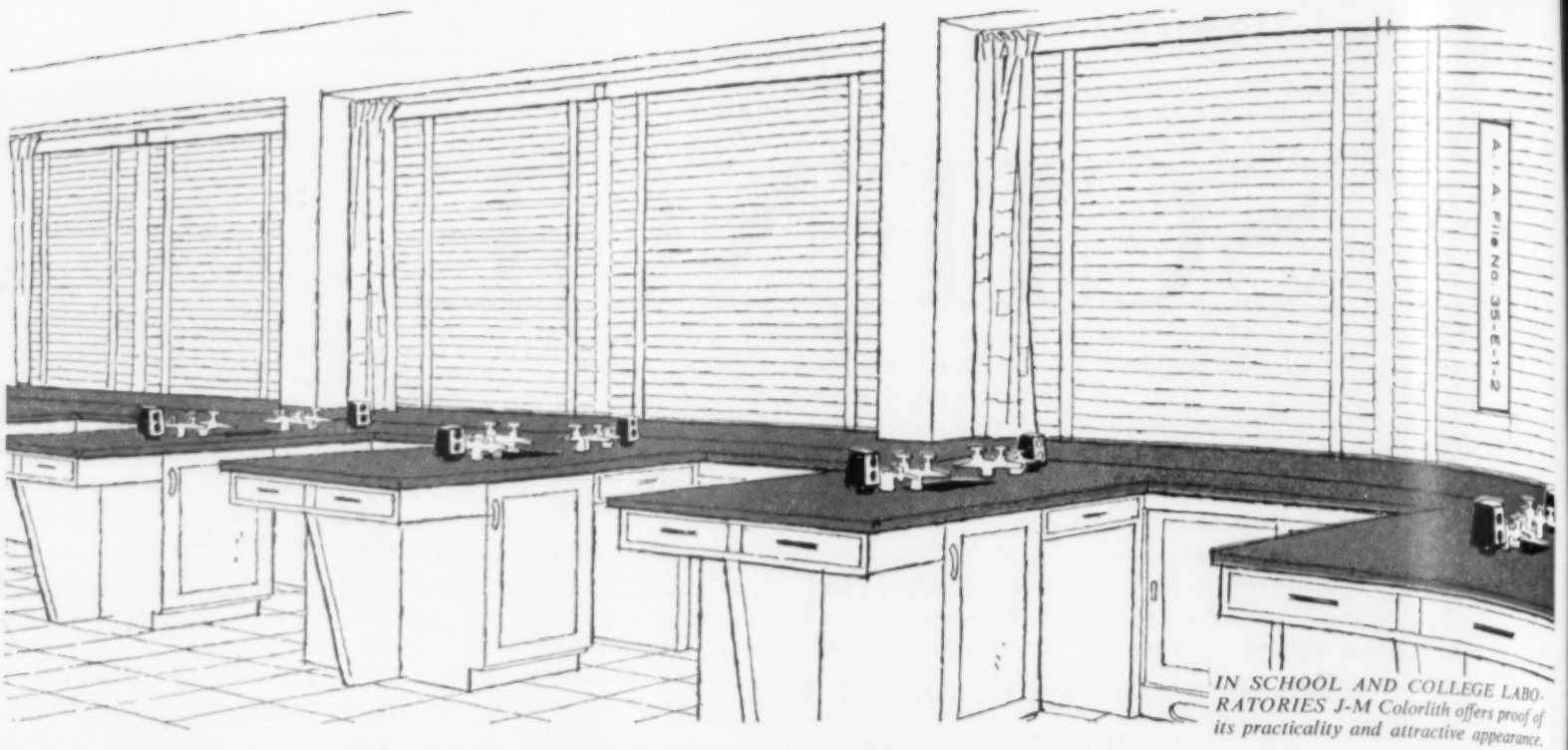

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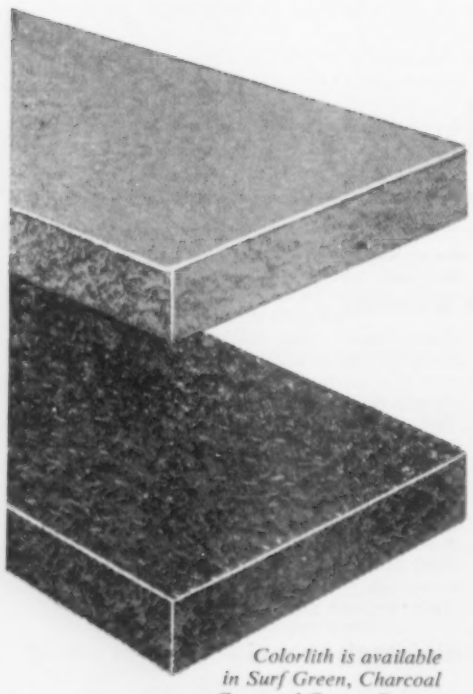

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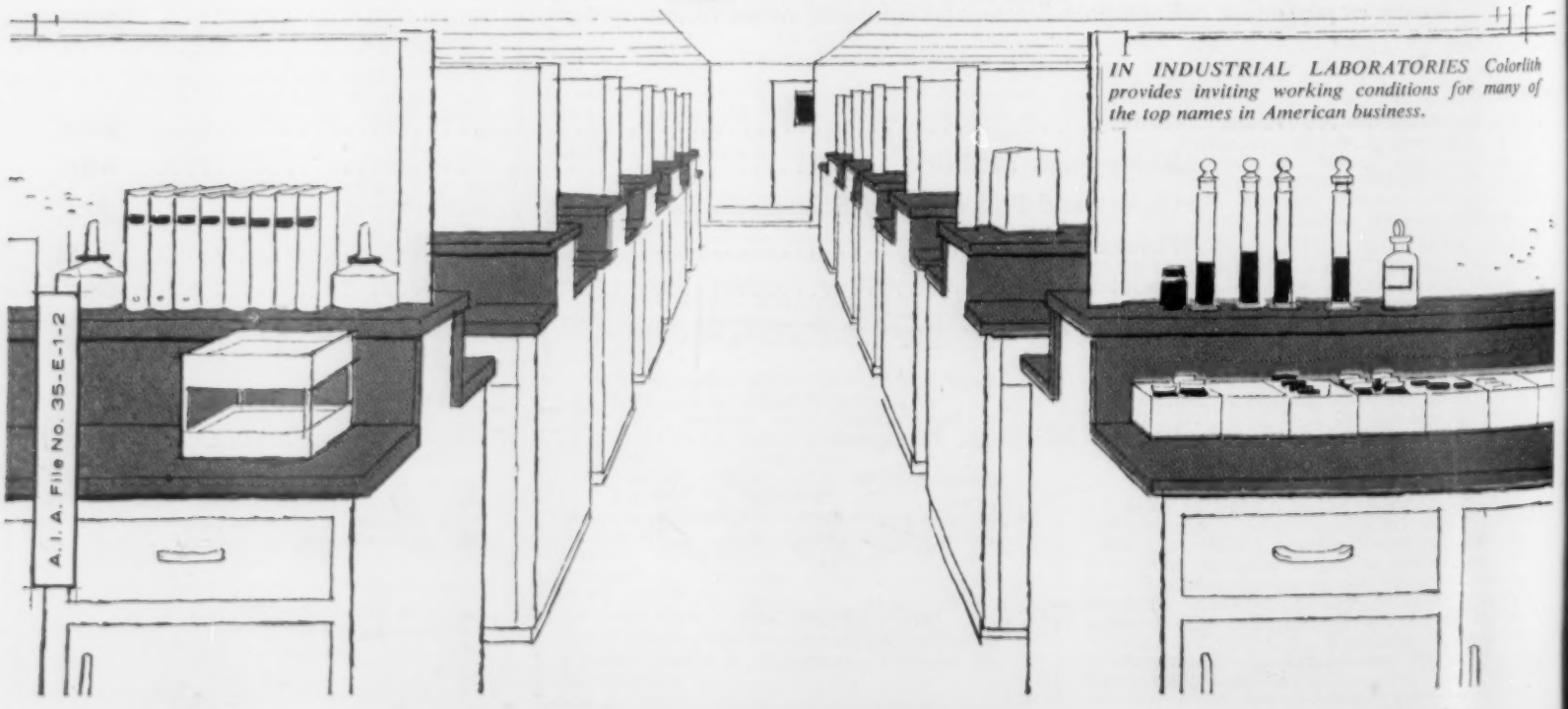

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Two Heads Better Than One?

A characteristic feature of the perennial call to action in behavioral science is the project for producing a unified theory of man and society by bringing together experts from various special fields. If the experts are given an extended opportunity for close communication, so the proposal runs, then they will successfully pool their separate bits of knowledge. Much hope is placed these days on team efforts, and there are occasions both in science and in other enterprises when a team effort has many advantages. However, there are also occasions when knowledge is not additive, when a team is no better than its best member.

The motivation for seeking a unified theory of behavior arises in part from a comparison with physics. The study of man and his institutions has been conducted for about as long as has the study of inanimate matter, but with somewhat less success. One peculiarity of physics is the way it develops around separate centers which are then combined to form comprehensive theories. But in behavioral science, despite the development of impressive techniques for measuring intelligence, attitudes, and other quantities, and despite extensive qualitative studies, efforts to effect the same kind of unification as is found in physics have not met with the same kind of general acceptance of results.

Although a comprehensive theory of behavior may be most desirable, we question whether a team project offers any special promise of success. And, by continuing the comparison with physics, we may be able to suggest why not. Consider, first, a rather different sense in which two sciences may be pooled—namely, the use of one science as a base of operations from which to assist the advance of another. An example from current research is the use of rockets to advance the study of the upper atmosphere. Here the team approach is clearly useful, but unfortunately there is no real unification. In fact, expert *A* can make use of expert *B*'s knowledge only because *A* does not need to know what *B* knows. Thus, the man who launches the rocket need not know anything about its payload, and the man who studies the upper atmosphere need not know anything about how his instruments got up there.

Now consider the problem of uniting the parts of science to form a comprehensive whole. The temptation again is to identify the men who know the parts and to unite *them*. But now this operation makes less sense, for to make the separate pieces of science stick together requires the cement of new scientific knowledge. Expert *A* can no longer make use of expert *B*'s knowledge, because *A* and *B* both ask the same question and neither knows the answer. Thus, suppose that in the middle of the 19th century a group of experts had been formed to tackle the problem of unifying the sciences of electricity and light. If the group had included James Clerk Maxwell, then surely it would have been successful—but then how necessary would the other members of the group have been? If the group had not included Maxwell, then to produce the electromagnetic theory of light, the group would have had first to come up with the equivalent of Maxwell's revolutionary hypothesis of displacement currents.

No doubt, at every level, from the family to international relations, there is a great need for the applications of a successful behavioral science. But without being branded as hopelessly old-fashioned, may we suggest that there are other ways to spend money in this science—ways which, if less dramatic than team projects of unification, are also more likely to succeed? Two heads may be as good as one, but they are not always better.—J.T.

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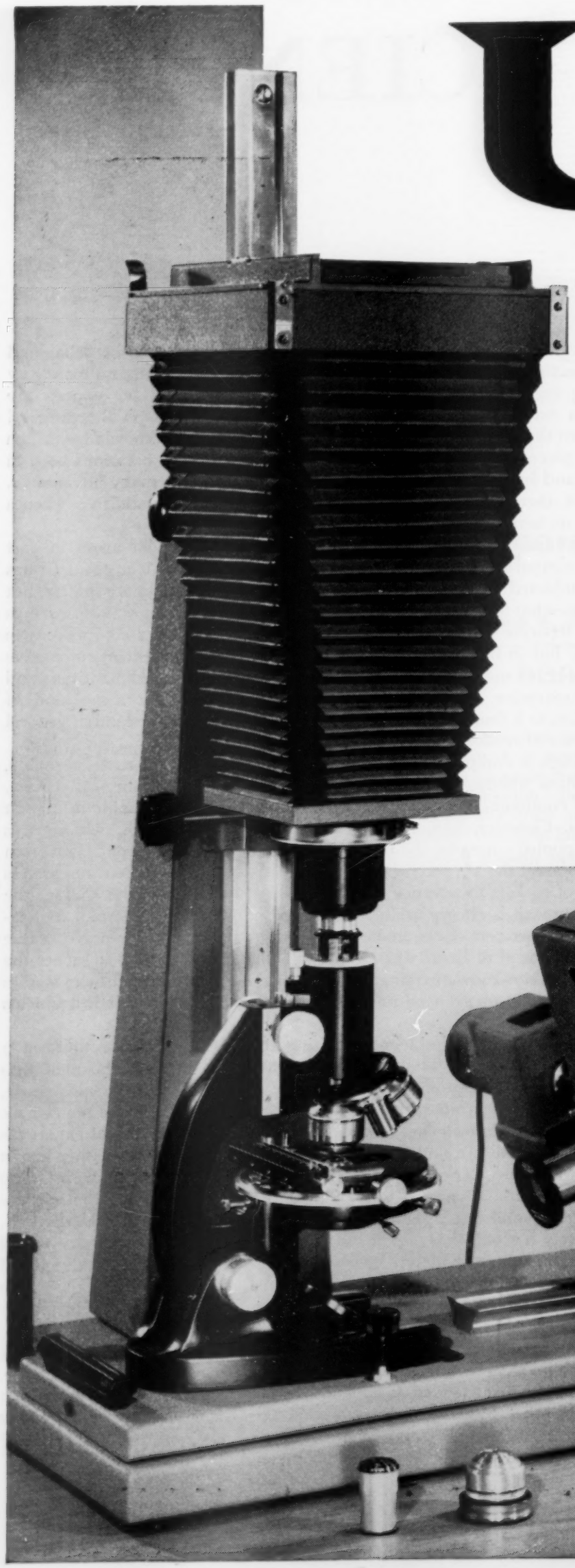

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SCIENCE

Dashing and Dutiful

Herschel and Maclear made a strange if effective team in their astronomical work at the Cape of Good Hope.

David S. Evans

The world is full of obscure people and obscure places which, when studied in detail and with sympathy, are seen to be worthy of a far wider fame than they enjoy. The two astronomers John Herschel and Thomas Maclear, well enough known, indeed, to the astronomical profession and "the Cape," where they worked together for four years, form an example of this.

The name "Cape of Good Hope" refers precisely to a most undistinguished point of land at the southern end of the Cape Peninsula, that appendage to the southwestern corner of the African continent whose northern end forms Table Mountain, with Cape Town nestling at its foot. "The Cape" also means, nowadays, the whole province of the Cape of Good Hope, which is—if the comparison is not impious—a little larger than Texas, and is one of the provinces of the Union of South Africa. Settlement of the Cape began in 1652 with the establishment of a "refreshment station" by the Dutch East India Company, for victualling their ships on the long haul round the Cape from Holland to their Far Eastern possessions. In time, this depot grew into a town, and the town into a colony, and the internal history of the region since that time has been one of steady expansion of the settled area into the interior.

Dr. Evans is on the staff of the Royal Observatory, Cape Town, Union of South Africa.

Early History

Even in very early days the Cape had a certain importance to astronomers. It was settled long before the marine chronometer made navigation at all certain, the weather in the region can be extremely rough, and it was by no means a difficult matter to miss the Cape completely, to find oneself lost in an angry southern sea. It became important, therefore, to fix the geographical position of the Cape as well as possible, by means of astronomical observations, and to observe the new constellations of the southern sky visible from it, to make them of use to navigators. Even before the establishment of any fixed observatory at the Cape, visits were paid by a number of astronomers, the most notable of whom was the Abbé de la Caille. Lacaille, as his name is usually spelt by modern astronomers, came in 1751 and spent two years in and around Cape Town, making astronomical and geodetic observations.

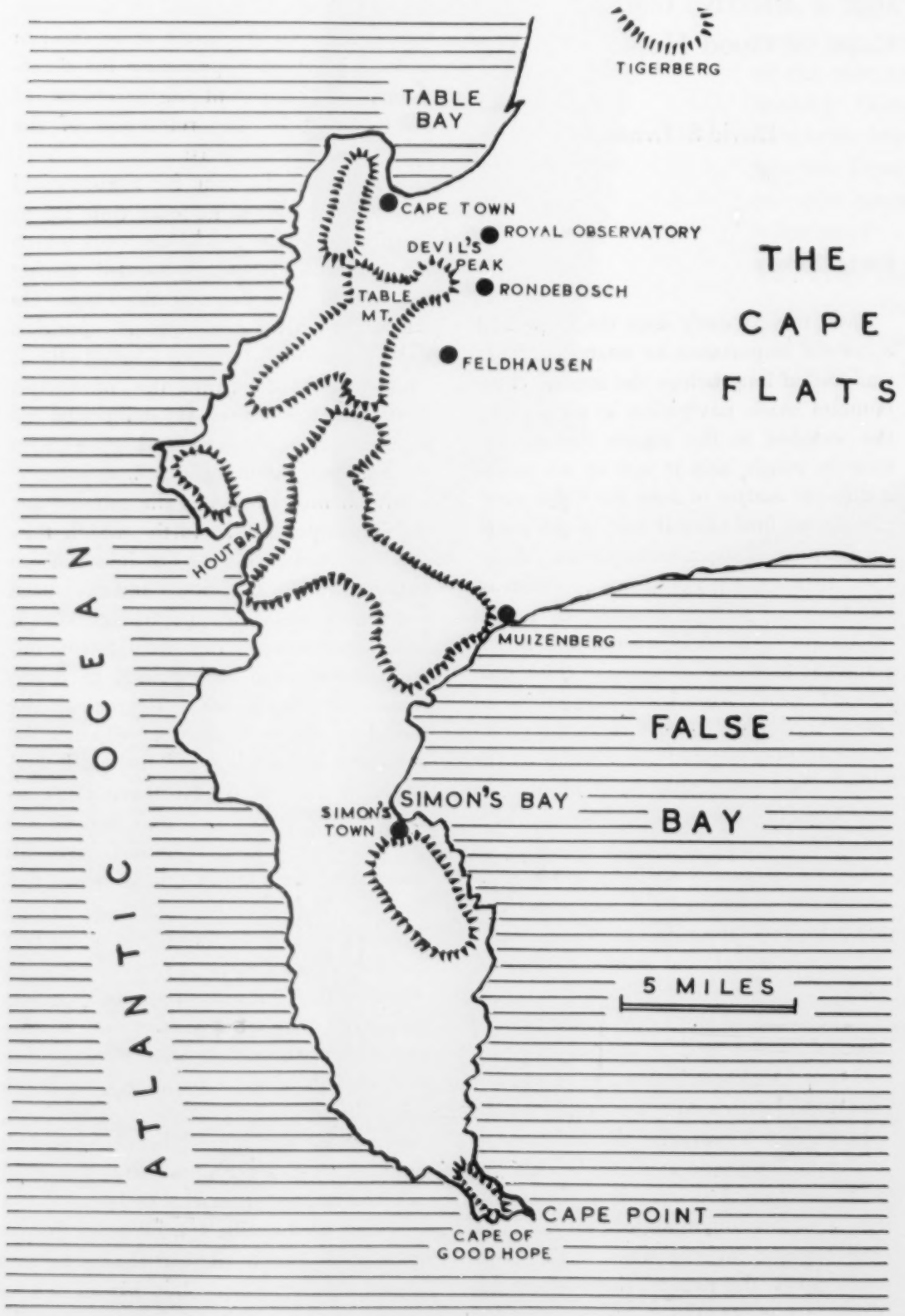
The choice of the Cape as the site for this expedition was a natural one, for not only was Cape Town one of the few European settlements far south of the equator but it was also the only one in the same longitude as Europe. This fact is one which has remained important for the development of astronomical and cognate investigations at the Cape, right up to the present time. When Lacaille came to the Cape, a problem which was exercising the minds of scientists in Europe was that of the exact shape of the earth, and, although it was thought that the Northern Hemisphere was oblate, nothing was known about the Southern. In addition, the establishment of two observing stations in nearly the same longitude provided a long base line which could be used to determine the distance of the earth from the sun and afforded an opportunity for determining the apparent displacement of stars caused by the refraction of the atmosphere of the earth.

Lacaille set the tone for astronomical investigations from his own time up to the present by the prodigious work which he accomplished single-handed during his stay of two years at the Cape. He determined its geographical position quite accurately. He used a small transit instrument to determine the coordinates of nearly 10,000 stars. He delineated the southern constellations and gave them the names of "philosophical instruments"—the chemical furnace, the microscope, the telescope, and so forth—which they still bear today. He set out into the unexplored north, laid down and measured a geodetic base line, and triangulated it into a north-south line with astronomically determined end points, to determine the length of a degree of the meridian and so to discover whether the Southern Hemisphere of the earth had the same shape as the Northern. His conclusion that it did not, and that it was a part of a prolate spheroid, gave his successors much trouble, until it was discovered that the attraction of Table Mountain at the southern end of his line caused a deviation of the vertical. Finally, this one-man research institute measured the average distance of the earth from the sun by determining what is called "the solar parallax"; although his value was about 10 percent too high, making the sun appear nearer than it is in reality, to have obtained a result at all was a most remarkable feat.

Most of Lacaille's work must be regarded as preliminary in character, and requiring repetition, but his choice of problems was so sound that much of the

later work has been concerned with the same list of problems.

Falling as it did under Dutch jurisdiction, the Cape was left a prey to invasion during the Napoleonic Wars when the Netherlands were overrun. The region was temporarily occupied by the British, after which there was a withdrawal and, finally, a permanent occupation in 1814. The British, as the leading maritime nation of the world at that time, were keenly alive to the strategic and economic importance of the two ports on the Peninsula: Simon's Town, the naval headquarters on False Bay on the eastern side of the Peninsula, and Table Bay, the port of Cape Town on the Atlantic seaboard, at the northern end. Even at this late date navigational methods were not beyond reproach, and the positions of the southern stars were still not known with the necessary accuracy. In 1820, therefore, an Order in Council was issued which led to the establishment of the Royal Observatory at the Cape of Good Hope "for the improvement of astronomy and navigation." It was one thing to give orders, another to get them executed. The first incumbent of the office of His Majesty's Astronomer at the Cape was the Reverend Fearon Fallows, a mathematician from Cambridge, with little practical astronomical experience. From the quiet of his English university town he was suddenly precipitated into a wild colonial life. Cape Town was still a very small town, and settlement in the hinterland amounted to no more than a few scattered villages and hamlets. Even so near Cape Town as the present site of the Royal Observatory, hippopotami and leopards were still to be found.



Cape Peninsula.

Fallows' first duty was to select an observatory site. To the east of Table Bay, but near enough to it to be visible from the decks of the ships anchored in the roadstead, there lay a little sandy hill, between two rivers, a desolate spot known as Slangkop, or Snake Hill. It was here that the observatory was finally erected, and here it still stands today, now in one of the inner suburbs of Cape Town. Fallows labored long and hard; finally he contracted scarlet fever and, in 1831, died of it.

His successor was Thomas Henderson, a man destined at first for the law, who became an amateur in astronomy and, finally, so much of an expert that he could be considered for the directorship of the Cape Observatory. Henderson was indeed a most remarkable man. He came to the Cape and remained only one year. He disliked the place intensely and found the desolate surroundings infinitely depressing. Forever after in his correspondence he would refer to the Cape Observatory as "Dismal Swamp," always with capital letters, and he was glad enough, in 1833, to return to Scotland, where he became the first of the Astronomers Royal for Scotland. He was on terms with his small staff which were none too good; he thought little of the quality of his instruments; and yet, in spite of the unreliability of these aids, Henderson, starting from scratch, was certainly the first man to measure the distance of one of the fixed stars, although, unfortunately, he did not publish the result soon enough to secure priority. At all events, he did not care for Africa (writing to Maclear in 1833 he said, "I will tell you about my residence in Dismal Swamp among slaves and savages—plenty of insidious venomous snakes. What would you think, if on putting out your candle to step into bed you were to find one lurking beside the bed?"), and his final resignation from the Cape was on a distinctly flimsy pretext.

It is at this point that Herschel and Maclear may be introduced, for both of

them arrived in the Cape early in 1834, Herschel as a private citizen undertaking an astronomical expedition, Maclear as the successor to Henderson in the post of His Majesty's Astronomer at the Cape.

John Herschel's Arrival

The two names have been described as obscure except to astronomers—an assessment which might be criticized, since those who know the name of Herschel are numerous enough. Unfortunately, the Herschel they know is not the Herschel of this story but his father, Frederick William Herschel, usually known as Sir William Herschel. He it was who began life as a bandsman in the Hanoverian Army, went to the fashionable spa of Bath in 1766 as a musician, took to astronomy first as an amateur and later as a professional, and became personal astronomer (Royal Astronomer, not Astronomer Royal) to George III.

It was William who was commemorated by Keats for his discovery of the planet Uranus ("Then felt I like some watcher of the skies when a new planet swims into his ken"), and it is William who is remembered by professional astronomers for his work on the development of reflecting telescopes, his studies of double stars, his determination of the spatial motion of the sun along the stars, and many other outstanding contributions. William began in obscurity and ended as one of the great celebrities of his age. Until the age of 50 he remained a bachelor, but, in 1788, he married a widow, Mary Pitt, and on 7 March 1792, their only child, John Frederick William Herschel, usually known as John Herschel, was born. Educated first at Eton and then by a private tutor, he went at the age of 17 to St. John's College, Cambridge.

John Herschel inherited his share of the family ability, and, in the distinguished environment into which his father had fought his way, he got off to a flying start. In 1813 he was senior wrangler, Fallows being third. He began his research career as a mathematician and wrote several memoirs. Then followed a brief period of legal study, after which the son, in collaboration with James South, reobserved the stars in his father's catalog of double stars. The Royal Society, the French Academy of Sciences, the Royal Astronomical Society, and other bodies showered honors upon him, and William IV knighted him. He repeated his father's survey of star clusters, using a 20-foot Herschellian "frontview" telescope—a form of reflector which has now become obsolete. Herschel had a good knowledge of chemistry and discovered the technique for fixing photographic images by means of "hypo." In 1826 he refused the chair of mathematics at Cambridge. In 1829 he married Margaret Brodie Stewart and eventually became the father of a large family. In other words, by the age of 40, John Herschel was a great celebrity, well endowed with this world's goods, a social lion, and a renowned scientific authority.

At some time in the early 1830's he made up his mind to undertake astronomical observations in the Southern Hemisphere, and it was this project which brought him to the Cape. Herschel's courage in undertaking a stay of four years at the Cape, and that of his wife in accompanying him with their family, should not be dismissed lightly. The voyage of 6000 miles was made in small sailing ships and took two or three months. It was in those days just about as tough a proposition to go to the Cape as it is in these days to go to the South Pole. This expedition was undertaken entirely at Herschel's own expense, and when, on his return, the British Government offered to reimburse him, he refused. While he was in South Africa, Herschel accomplished a tremendous work. In 1838 he returned to England, where Queen Victoria, at her accession, created him a baronet, and he was accorded a rapturous welcome. But strangely enough, after his return, Herschel never put eye to telescope again, and it was some years before he published his account of his work in a large volume, *Results of Astronomical Observations at the Cape of Good Hope*. Pritchard, in his biographical note on Herschel published in the *Encyclopaedia Britannica,* said, "Herschel had become an astronomer from a sense of duty, just as his father had become one by fascination and fixed resolve: hence it was by filial loyalty to his father's memory that he was now impelled to undertake the completion of that work which at Slough had been so grandly commenced. William Herschel had explored the northern heavens: John Herschel determined to explore the heavens of the south, as well as re-explore the north."

This judgment seems rather harsh. After all, many a man who has done much less than Herschel for the advancement of science is accorded ungrudging recognition. Yet, it is a justified assessment of Herschel, and all estimates of him must include this note of criticism. His was a complex character. He possessed ability of an outstanding order. He cannot be called a dilettante, for dilettantes do not best all comers in the mathematics tripos, nor do they undertake long and arduous research programs and bring them to successful conclusions. To have done, superbly, everything that he could do—in mathematics, astronomy, optics, chemistry, drawing, administration, social life—would have made Herschel a Leonardo or a Goethe, and such a man he was not. To sum up his character in a sentence, one might say that what he lacked was single-mindedness, and if he had had it, he might have been a genius.

Sir John Herschel.

As it was, the lack of need to fight as his father did dissipated his efforts, and his success at most things blinded him to the possibility of being outstanding at any one. From the beginning he had social graces and urbanity. In a hard-drinking age he was not reckoned anything but abstemious, but he could, on occasion, enjoy the pleasure of the table more than was wise. Never having been a professional, he lacked the tough-mindedness which carries the seasoned campaigner through moments of stress, and having been born with a silver spoon in his mouth, he did not encounter many such moments. Yet, in the end, he was a man of qualities, and the possession of these qualities turned out to be crucial for the years of his association with Maclear and provided the latter with the complement to his own personality necessary for his own success.

On 15 January 1834, the *Mount Stewart Elphinstone,* East Indiaman, put

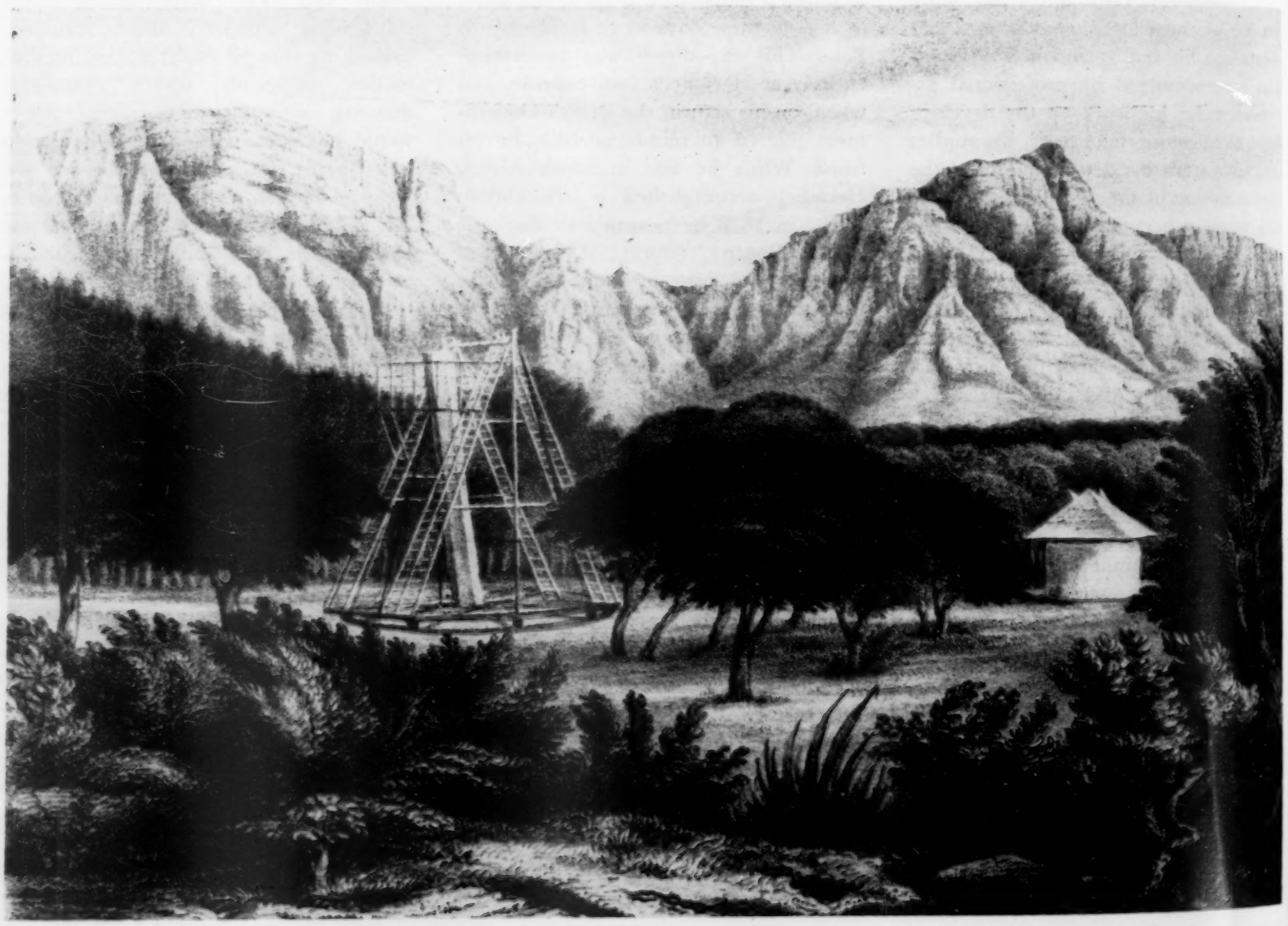
ashore her passengers at Table Bay, two months out from England. Among them were John Herschel and his wife and the four children—Caroline, Emilia Mary, William James, and Isabella—and the mechanic John Stone. In the baggage was a reflecting telescope of 20-foot focal length and a diameter of 1½ feet and what Herschel called, with a classicist's joke, three "sesquipedalian" speculum metal mirrors, to be used in it in turn. In addition he had an equatorially mounted refractor of 5-inch diameter and 7-foot focus. After a short stay in a private house, the Herschels moved to a farm called "Feldhausen," a Cape Dutch-style residence in extensive grounds just to the east of the Table Mountain massif, a property which Herschel purchased in the next year for the sum of £3000 (representing, probably, an amount not far short of $50,000 in terms of modern purchasing power). The estate (which was at that time sometimes also known as "the Grove") has been split up and encroached upon by building, but the house still remains, and the site of the reflector is now marked by a singularly ugly stone obelisk.

Astronomically speaking, the site is not particularly good. The annual rainfall in and around the Cape Peninsula varies enormously with situation relative to the mountains. Feldhausen probably has a rainfall (mainly winter rain) not far short of 50 inches per annum, whereas the Royal Observatory only four miles away, gets rather more than half of this. In addition, the western sky is cut off by the mountain wall, and this proved a handicap to Herschel in his observations of comets. However, at Feldhausen Herschel established himself for the four years of his stay. The 20-foot reflector was ready by 22 February, the equatorial by 1 May. In addition, he had a portable telescope which had been used by his famous aunt, Caroline Herschel, at Slough. A small cottage in the grounds became a workshop and polishing house for figuring the mirrors and a chemical laboratory for experiments, including some on the color of flowers.

Maclear's Arrival

The career of Thomas Maclear, up to the time of his arrival in Africa, could hardly have been in greater contrast. He was born in Ireland in 1794 and destined originally for the Anglican Church. Against this he rebelled, and, from the age of 15 years, he received a medical education under the supervision of two medical uncles, Sir George and Dr. T. Magrath. After completing his studies he became house surgeon at the Infirmary at Bedford, where he came in contact with the Smyth family, who were to exercise so strong an influence upon him. In 1823 he moved a few miles, to Biggleswade, where he practised in association with one of his uncles. In 1825 he married Mary Pearse, and in 1833 he was appointed His Majesty's Astronomer at the Cape, to succeed Henderson, whom he already knew well.

Obscure country doctors were not, even in the early 19th century, appointed to public positions without qualifications,



Feldhausen in Herschel's time, with the 18-inch reflector and Devil's Peak in the right background.

and the transition from medicine to astronomy requires some explanation.

The circumstances of the meeting between Maclear and the Smyth family, and their friend Dr. Lee, are a little obscure, but the period is that of Maclear's sojourn in Bedford, where the head of the Smyth family, most readily distinguished as Admiral Smyth, was then living. England has a reputation for nurturing and tolerating engaging eccentrics, and Admiral Smyth was certainly one such. His father was an American who sided with the British at the time of Independence. Smyth himself served in the British merchant navy, and then in the Royal Navy during the Napoleonic Wars. He held command in the Anglo-Sicilian fleet at Messina and, finding himself there, made hydrographic surveys of the area on his own initiative, a proceeding of which his superiors fortunately approved. In 1815 he married the daughter of an English merchant in Naples, and he also met the Italian astronomer Piazzi. From that time onwards, Smyth devoted himself to astronomy as an amateur, and when his second son was born, he named him Charles Piazzi Smyth—a name which we must keep in mind, for Charles Piazzi Smyth was eventually to become assistant to Maclear at the Cape. Astronomy did not absorb all the energies of the admiral, and he produced a large illustrated work on old coins and medals. This too is of importance, for the antiquarian interests of the father were probably the starting-point for Charles Piazzi Smyth's obsession with the Pyramids of Egypt—the son was, indeed, the originator in later life of all the Pyramid cults, some of which still flourish.

Admiral Smyth, returning from the wars, settled down to a period of retirement at Bedford and devoted himself to astronomy as a hobby. He became prominent in the affairs of the Royal Astronomical Society and, about this time, became an intimate friend of Dr. Lee, a wealthy lawyer, who was also an amateur astronomer. Lee eventually bought Smyth's instruments and set them up at Aylesbury, yet another of the small towns in the more distant outskirts of London.

Astronomy bit these well-to-do amateurs as Hi-Fi might bite them today. Smyth produced a quite good two-volume description of astronomical objects in the northern sky, under the titles of *The Bedford Catalogue* and *A Cycle of Celestial Objects*, interlarding the astronomy with some very odd stuff in-

Mr. (afterwards Sir) Thomas Maclear. [By permission of Her Majesty's Astronomer at the Cape]

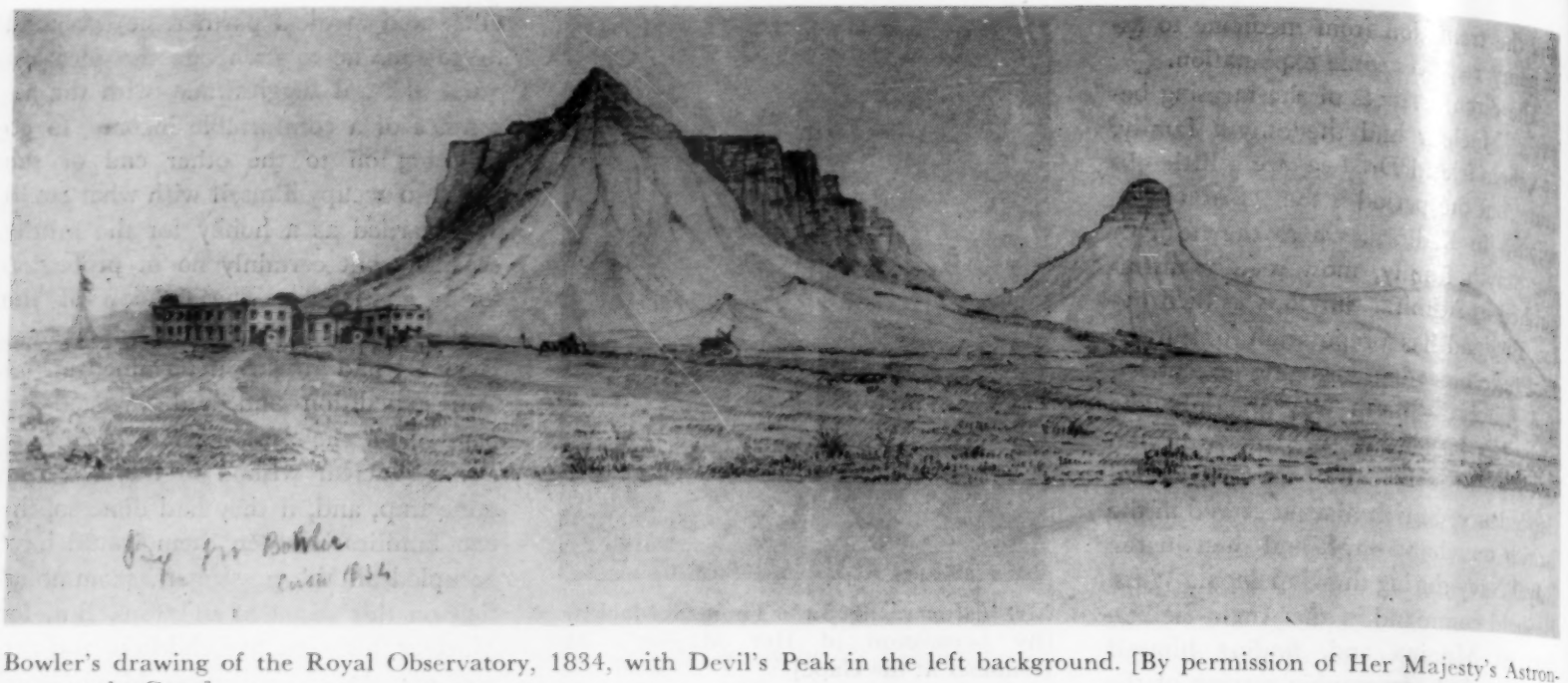
deed. As one reviewer put it, "The descriptions of the various objects are enlivened with a vast amount of general classic and antiquarian lore, introduced in a most genial spirit." Smyth and Lee could afford the time for these activities, but it remains a mystery how Maclear could earn his living as a doctor (and command the affection of his patients—they got up a subscription and presented him with a piece of plate when he went to Africa) and still find time for all his astronomical activities. He observed eclipses, the phenomena of Jupiter's satellites, and so forth, just as any amateur may do today, but he also ran a transit circle and determined star positions and, in cooperation with Smyth and Lee, made extensive geodetic traverses. He became a member of the Royal Astronomical Society and found time to run up to London to attend meetings, at which, no doubt, he first made the acquaintance of such men as Herschel and Henderson. Eventually, when the post at the Cape fell vacant, Maclear was an amateur of sufficient distinction to be considered for it. It must, however, be acknowledged that there was, in all probability, little competition. The remoteness of Africa and its uncivilized condition were two of the discouraging factors. Moreover, the history of the Observatory itself cannot, at that date, have seemed very rosy. In 12 years there had been two directors, and, if rumor was to be believed, not only were some of the instruments far from reliable but there were difficult staff questions to be resolved as well.

In the eyes of his contemporaries and, more especially, in the eyes of Maclear's uncle and medical partner, he was mad to go—insane to exchange the pleasant quiet of rural Biggleswade, with the assurance of a comfortable income, to go traipsing off to the other end of the world to occupy himself with what could be regarded as a hobby for the mildly eccentric but certainly no fit profession for a gentleman. The opinion of the uncle was of very practical importance. Herschel had already determined on his own expedition and had engaged his passage on the *Mount Stewart Elphinstone*. Maclear wished to travel on the same ship, and, if they had done so, the two families between them would have occupied all the passenger accommodation on this vessel of 611 tons. But, for Maclear, his wife, five children, and two servants, plus the governess, Mary Geard (whom they forgot in getting their first estimates), the passage money would be £350. This was big money, and, as Maclear did not possess such a sum, he turned to his uncle and partner, only to meet with a brusque refusal to supply a single farthing.

In the meanwhile, Admiral Beaufort, Hydrographer to the Navy, was busy on their behalf and, in September 1833, wrote that he had a passage for Maclear in a government ship for £36. An interesting sidelight on travel conditions of the time is thrown by Beaufort's letter to Maclear, which urges him to "come up on Monday to see your cabins and order your pigs and chickens and potted meats" and recommends that he take a store of well-dried potatoes and apples as antiscorbutics. Finally, the whole party embarked at 8 in the morning of 10 October 1833 on the *Tam O'Shanter*; in spite of the good report on the captain given by a friend, whom, with naive prudence, they had had inspect the master mariner as he drank in a tavern, they were all hideously seasick, and one of the children died.

The Assistants

Their relief as they stood on the deck of the *Tam O'Shanter* on the morning of 5 January 1834 and looked at Table Mountain must have been immense. Maclear must have been only too sensible of the fact that new problems awaited him, and even more concerned by the discovery that his so-called "free passage" to South Africa was going to cost him, according to some strange contemporary formula, the staggering sum of £220.



Bowler's drawing of the Royal Observatory, 1834, with Devil's Peak in the left background. [By permission of Her Majesty's Astronomer at the Cape]

What the passengers thought at that moment is not entirely a matter of conjecture, for one of them, a manservant of Maclear's, has come down to us as a minor historical character, possibly better known in South Africa than Maclear himself. This was one Thomas Bowler, who was recommended to him by the wealthy Dr. Lee. Bowler was the grandson of one of Lee's housekeepers; evidently he had several houses and several ladies to keep them, for in a letter of 7 December 1833, John Hawkins, brother-in-law of Mrs. Maclear, who was looking after the Maclear interests in England, wrote, "Dr. Lee has taken to wife a lady who until that time fitted the important situation of Housekeeper . . . I have not heard more particulars but conclude it is not the grandmamma of your servant nor is it the one who lives at Colworth."

Bowler's connection with the life he had lived in England made Maclear at first regard him as a stable and reliable element in an uncertain world. Maclear had need of trustworthy colleagues, for, when he arrived at the Observatory, he was met by Henderson's assistant, Lieutenant Meadows. Under Henderson, Meadows had worked well as an observer, but, by the time Maclear had arrived, he was a soured and dissolute individual maintaining a *ménage à trois,* who greeted his new chief with what must have been one of the coolest welcomes on record, "So, Sir, you have determined to accept this wretched appointment." From this auspicious beginning they never looked back, and it was not long before the lieutenant and his entourage were on their way to England.

Thus Maclear was more or less forced to rely on Bowler and would send him down to Simon's Town to draw Observatory funds, or to carry out other responsible tasks, and after some time Bowler was given the vacant post of "Labourer" and even contributed a little to astronomical observation. The honeymoon did not last, and Bowler's behavior became wonderfully awkward. At first he lodged in the East Wing of the Observatory and, by contemporary standards, could have been fairly comfortable. When Maclear dismissed him he felt so much responsibility for him that he wrote a full account of the affair, both to Dr. Lee and to the clerk at Simon's Town, one Mr. Deas Thompson. The Labourer, said Maclear, might "boil his own kettle morning and evening, and cook his own steak. He can be as comfortable at the Observatory as anywhere. The butcher and the baker come every second day." Bowler got £70 a year, the usual salary for a clerk. Boarding-houses at the Cape charged high, and a careful fellow could board for himself. "A 10 lb fish is 2d or 3d, butcher's meat 1½d to 2d per lb. Bread, vegetables and butter as dear or dearer as in England. Bowler is steady and if he can acquire an obliging disposition to his employer he will do well." This Bowler could not do, and, although Maclear boarded him free out of his own pocket for a time, Bowler insisted on taking his meals in the village of Rondebosch and was, in consequence, absent for two hours or more three times a day. Eventually he insulted Mrs. Maclear in some way, and Maclear turned him out. He kept himself by instructing the daughters of neighboring farmers in that mysterious polite accomplishment—one wonders how often it was made use of—"the use of the globes." Already, while at the Observatory, Bowler had begun to draw, and his first sketch was one of the Observatory and its surroundings in 1834. Bowler went on to become a professional artist, and modern knowledge of the contemporary buildings of Cape Town and, indeed, of life in most parts of the Cape, owes much to numerous drawings and prints which Bowler afterwards produced. Although inconsiderable as an artist, Bowler was important, and almost alone, as a recorder, and his work now fetches a considerable price.

The tale of Maclear's assistants is completed by mention of the arrival, in 1835, of Charles Piazzi Smyth, son of the old admiral, who came as a boy of 16. He, at least, was biddable, hard-working, competent, and enthusiastic and no mean astronomical artist. He remained with Maclear for many years, eventually leaving to become Astronomer Royal for Scotland. His later distinction in spectroscopy and in his attempts to observe the solar corona in daylight from the summit of the Peak of Teneriffe, as well as his eccentric preoccupations with the Pyramids of Egypt and their capacity for the prediction of the destinies of the human race, belong to another story.

These were only some of the difficulties which Maclear had to overcome, and the fact that he did overcome them and made of the Royal Observatory the considerable institution which Sir David Gill was later to raise to eminence, is a

measure the toughness of his character and his devotion to duty. That Maclear was tough, there is no doubt; had he been less severe and less determined he would have failed. In later years his staff recognized both his inflexibility and the need for it and, among themselves, would refer to him as "the Emperor," while, on Maclear's retirement in 1870, his virtues were so widely recognized and he was so well-loved a figure that the Colonial Legislature passed a special resolution to mark the occasion, and he was accorded the same kind of rapturous public welcome that Herschel had received on his return to England from South Africa.

Yet another personal difficulty faced Maclear on his arrival at the Cape. Here he was, in an isolated community where medical care was, no doubt, primitive even by the elementary standards of the early 19th century, a qualified doctor professionally employed in a completely different field. Maclear himself recognized the difficulties which might face him. In his diary for 1834, on 5 February, he wrote, "I am afraid of dipping into Physic," but, to a large extent, he did manage to stick to the new last, to the total exclusion of the old. He treated himself, dosed the Meadows family, drew a tooth for Lady Herschel, and amused himself on one occasion by dissecting a snake which had been killed in the Observatory grounds. Perhaps his zoological interests were somewhat wider and better known than this single incident would indicate, for stray characters would occasionally appear with snakes in varying degrees of liveliness which they hoped to sell to Maclear.

There are few better examples of Maclear's devotion to duty, of the horrors of old-style medical treatment, and of the relations between Maclear and Herschel than the following incident, which begins with the entry in Maclear's diary for 10 April 1834. Maclear writes:

> Remarked a peculiar sensation in my mouth on the right side—the feeling of a crumb of bread or something in it. When I blew my nose the cheek filled out and became inflated, while the left preserved the usual muscular resistance so that to do this effectually I am obliged to push in my cheek with my hand. There is no loss of sensation. . . .
>
> 10 o'clock. Took a dose of Epsom salts. On exam^{g} my face in the glass I find that on laughing the integuments are drawn to the left side and in masticating food the space on the right side between the gums and cheek is filled but I cannot without the pressure of my hand empty it. I cannot effectually close my right eye. For some days I have been working hard with the mural circle. My left eye became tired and painful & 2 days since was rather inflamed, with a pain shooting into my head.

Then follows a blow-by-blow account of his struggle with his facial paralysis, which lasted until the end of the month. The regime of purging, cupping, blistering, and application of leeches which he adopted seemed to be based on the idea that the illness would give up in disgust if assaulted in every conceivable way. Sir John was distressed and sent him a consolatory letter in his customary style. Herschel's ideas of consolation ring oddly in modern ears, but friendship survived even this. He wrote:

> I am sorry to find your bulletin speak of no improvement but '*courage mon ami*'—if the worst comes to the worst, a twisted visage may be put up with. In the case of a bachelor on his preferment, it might be a serious thing, but once family taken 'for better for worse' and estimated and loved for qualities more than skin deep a man may keep the laugh on his side though it may be only on *one* side,
>
> Adieu.

Herschel and Maclear

Much of our knowledge of the personal relations between Herschel and Maclear depends on the extensive correspondence, usually Herschel's side only, which is preserved in the South African archives. Herschel's epistolatory style was so remarkable that one is tempted to linger over it far too long. He often wrote the most excellent sense, but in doing it he equally often behaved like a ham actor. He gestured, he underlined, he exclaimed, he made little academic jokes, and he dragged in quotations in all the numerous languages he knew. He indulged in florid turns of speech, he condescended amiably. When he was excited—which was often—his handwriting was atrocious, and he abbreviated his words to such an extent that it is sometimes almost impossible to make out his meaning. Here, for instance, is a brief note which he wrote to Maclear on an occasion when he was engaged—actually with the assistance of Bowler—in making tidal observations in Table Bay.

> Thursday Afternoon
>
> My dear Sir,
>
> I have made so dreadful a *mess* of the Tide Ob^{sn} I took on *Monday* as well as of the Meteorological Ob^{svg}, by using Daniel's watch —as defies all my power to decipher—i.e. in respect of time. It is something so compounded of the ludicrous & the melancholy as to be an epitome of the great tragi-comedy of human life. In truth I am ashamed to shew it to you—yet as a matter of curiosity and philosophical enquiry it may be worth while. I shall therefore ride over to the Observatory between the High and Low water tomorrow and "make a clean bosom" of the whole affair.
>
> Tomorrow (Friday) the stroke of 8 will find me on the jetty. If you will be up, I think I shall take the Observatory in my way and pick up a time I can depend on. My blunders (if blunders they were—i.e. if the evil one had not his finger in the pye) went to whole hours —half hours and quarters. It is something enormous—incredible—and to me utterly incomprehensible,
>
> Yours in haste,
>
> J. F. W. Herschel.
>
> P.S. On second thoughts I will not come via the Observatory. There will be too little time and I shall miss the Observations altogether most likely by so doing.

A laconic note in pencil is added to this letter in Maclear's handwriting, "A fog. Missed his way and found himself at Tyger B. T.M." Tigerberg is about five miles away from the Observatory in the wrong direction.

It is surprising that the mutual affection of the two men survived some of the assistance which Herschel was ready to render. Their relations always remained warm, but the nature of the relationship changed. When Maclear first arrived he was almost overwhelmed by immediate obstacles, and the moral and practical support of Herschel undoubtedly sustained him. As Maclear's self-confidence grew, it was he who became the more active and professionally tough-minded of the two. Herschel tended to retreat to the side lines and to offer advice, some of it good, some impractical.

For example, in 1837 Maclear set to work to measure a geodetic base line on low-lying ground near the Observatory. This was done by laying rods supported by trestles end to end and measuring the gaps between. Herschel certainly helped a great deal with the preparatory work, and the first attempts began in March. On 12 March Herschel felt constrained to write from Feldhausen:

> My dear Sir,
>
> I feel so thoroughly the need of a day's rest after the Met Obsn^{s} that I must beg you to excuse my not keeping my app^{m} tomorrow (Thursday) but will . . . come on Saturday (the other being Good Friday)—yesterday's work was too much for me the Therm the whole day being at & above 90 and being exposed for some hours out of doors has produced a degree of over excitement which makes me fear another fit of sinkings something of which I now begin to feel.
>
> I send specimens of lines engraved on mica with a steel needle—Glass does not satisfy me as it is so difficult to get a *scratch* which will not be a *cut* nor turn up at the edge—I think mica will do . . . not having the dimensions I have drawn the lines at random,
>
> Yours truly,
>
> JFWH

The mica scales were for measuring the interval between the measuring rods, and were no doubt of real assistance. For all his verbosity, Herschel was quite willing and capable of tendering excellent advice, some of it based on the most lengthy and precise cogitation. The

"Met Obsns" referred to are another matter in which Herschel seems to have been an innovator. In addition to ordinary daily observations taken at one or two fixed times in the day, he proposed that on four days in the year observations should be taken throughout the 24 hours, and this he proceeded to do unaided. The records which he made every hour from the morning of 20 December 1835 until the evening of 22 December have been preserved, having been copied out by Piazzi Smyth for presentation to Dr. Lee's observatory at Hartwell. Lee made a note on the copy to this effect, and, by a coincidence, the document has now found its way back to the South African Library in Cape Town.

To revert to the measurement of the geodetic base line: By June of 1837 work was well under way, and present with Maclear were Sir John, Lieutenant Williams of the Sappers, and Piazzi Smyth. After one day, Herschel had a cold but came to visit. During the operations the wind blew over one of the rods, and the work was then held up until November because the low-lying ground became flooded. Herschel's idea of consolation ran:

> My dear Sir,
> I am sorry for the accident but I confess I was at no time sanguine as to the completion of the base without some interruption of the kind. The rods are top heavy and in case of any new operations commenced here with such instruments I should urge on the attention of those concerned a thing which if you recollect I did at one time mention as no imprudent precaution. . . .

(Then he goes into details of a plan for adding kingposts, braces, and stabilizing weights to the rods and, after signing himself, adds, "P.S. The first 700 feet of the Irish base were obliged to be remeasured by an accident similar to yours.") Herschel never confined himself to one letter if two could be written, and the following undated note must refer to the same incident:

> I feel today so great an increase of certain very unpleasant sensations which were creeping over me the whole of yesterday, that I hold it very doubtful whether I shall be in a condition to come over tomorrow or at all events for more than an hour or two in the warm part of the day—indeed I begin to perceive that another entire day's duty in your swamp will go nigh to lay me up for the winter, and I cannot help feeling that there are many days' work before that measurement will be completed—to say nothing of some unlucky jog which may render it necessary to remeasure the whole.

One of the accompanying illustrations, which is reproduced from an old print, shows that Herschel's ideas for the measurement of a geodetic base line were adopted by Maclear. The print shows Maclear laying out a standard base line on the Grand Parade in Cape Town where it might be used by surveyors for checking their tapes and chains; in the background can be seen the measuring rods, complete with the kingposts and braces lying on their supporting trestles.

Making Surveys

This may be a convenient point at which to summarize the general program of survey work undertaken by Maclear. It begins at the point, already mentioned, where Lacaille had concluded that the Southern Hemisphere of the earth was a prolate and not an oblate hemisphere. Between Lacaille and Maclear, Everest, of the Survey of India (he after whom the mountain is named), had spent some time at the Cape trying to pick up Lacaille's survey points to check his work, and one of Maclear's most important assignments was to do this again, and more thoroughly, and to extend the work. Maclear spent much time in his first years at the Cape exploring the town and its surroundings, identifying the site of Lacaille's observatory and trying to recover his stations. In this he was not completely successful, for the trail was 80 years old, but he did find a certain number of the points used by Lacaille, even, in some cases, finding the ashes of the signal fires which his predecessors had used. After this preliminary work Maclear, assisted by voluminous correspondence and discussion with Herschel, began to examine methods which he might adopt in his work. (In this International Geophysical Year it is interesting to look back and to see how the pair anticipated some of the most up-to-date geodetical methods, which are now practicable because of improved equipment.)

The connection of surveys across sea barriers is still a major difficulty today. Maclear and Herschel were not concerned with sea barriers, but they had in mind the possibility that they might have to carry a survey across inaccessible country.

They therefore experimented with rockets fired at Muizenberg, observing them from the Observatory and from Simon's Town, the object being to connect the two places without having to survey the intervening ground in detail. Much the same idea was behind the observations of lunar eclipses from Feldhausen and the Observatory. The observations consisted in timing, as exactly as possible, the moments when the eclipse shadow arrived at particular craters on the moon, and they hoped that they might be able, from these observations, to determine the relative positions of their two observing stations with great accuracy. That Herschel and Maclear failed was due to no error of principle; when they found their methods insufficiently accurate, they turned to more conventional ones.

In chronological order, Maclear's work in surveying and geodesy may be summarized as follows: First came the historical investigation, with the object of repeating what Lacaille had done; then came discussion of methods with Herschel. Serious work had to wait until Maclear was provided with assistance by the arrival of Piazzi Smyth, in 1835. Toward the end of 1836 Maclear first measured a base line on the Grand Parade in Cape Town, a little while after he had first commenced survey operations near the Observatory. The abortive base line was spoiled by floods, and improvement of the measuring rods came in 1837. The Parade base line, having been, presumably, inadequately marked, was remeasured on 2 December 1837. The base line near the Observatory had already been completed a few weeks before, by means of the improved measuring rods.

Herschel, not to be outdone, had meanwhile measured a little base near Feldhausen and surveyed in some of the surrounding landmarks. An almost indecipherable letter, written in the dark on 3 December 1837, expresses his relief at having missed the work on the Parade:

> Your expedition in matters of business puts to shame my tardiness & proves to me that I am getting hardly fit to inhabit a bustling world like this. While I speculated on how long is to take to get the guns—how long to get them sunk—how long to take up and sink again in consequence of detected errors—and how long to get all in order—behold your Parade base is measured—as it is I am rejoiced that I have *escaped* being present. That *broil* would have *done* me thoroughly. The next thing I expect to hear of is Messrs Maclear, Smith Williams & Co—ill of brain fevers. . . .

From this point on, Maclear moved into the field of scientific geodesy. He was not able certainly to recover Lacaille's marks, and in the end embarked on what became, first, an independent local survey, leading many decades later, long after his time, to the establishment of the official government Trigonometrical Survey Office of South Africa.

For his scientific work Maclear obtained the use of a historic instrument, the zenith sector which had been employed in his researches by Bradley, the

discovered the aberration of light. The idea was suggested by Airy, the Astronomer Royal, and in 1837 this monstrous instrument was shipped out and reassembled. Monstrous is the only possible descriptive adjective, for, when set up for use, this contraption required a tent 17 feet high to house it and protect it from the wind. Year after year, Maclear, Smyth, and others lugged this piece of equipment and its wigwam over uncharted and roadless country for hundreds of miles, up to the tops of mountains and across rivers and ravines. Most of the duplication of Lacaille's work was completed before the departure of Herschel, in March 1838; during this time they went as far as the Piketberg region, about 100 miles north of Cape Town. In the years immediately following, they reached as far north as the mouth of the Orange River, still a scantily populated and remote district, where one of the mountains is named—no doubt mysteriously in the eyes of the present inhabitants—the Sector Berg. Maclear later gave an account of these proceedings, omitting the racy correspondence of Herschel, in an official Observatory publication, *The Verification of Lacaille's Arc of the Meridian*, published in 1867.

Cataloging the Southern Sky

The principal object of Herschel's journey was to discover and catalog the nebulae and double stars of the southern sky. This was done by the method of sweeping devised by his father. It consisted in setting the telescope at a particular declination and making notes of the objects which appeared in the field as the sky was carried past by the rotation of the earth. Objects of special interest could then be given detailed observation later. Eccentric though Herschel's character was, it would be grossly unfair to him not to emphasize that he carried out this program very efficiently and that, even to this day, there are many cases where his observations remain the only ones. Herschel has left his record in the *Observations at the Cape of Good Hope, 1834–38*, which has already been mentioned. His catalog of nebulae is probably superior to his catalog of double stars, for the latter contains many examples of wide, optical (as distinct from physically related) pairs. His catalog of nebulae comprises not only what we would now call extragalactic ("spiral") nebulae, which are genuine large-scale independent systems resembling the Milky Way system in size and constitution, but also all the details of our own Milky Way system which he observed, conspicuous only because of their

IV.
Measurement of the Base-Line on the Parade Ground in Cape Town
1. Mr Maclear. 2. Major Mitchell. 3 Lt Williams. 4 Mr Hertzog. 5 Orbit Circle. 6 Richt Ascen. 7 Policemen, keeping the ground.
8 Cape Spectators. 9 Indian Spectators 10 Members of an Extra-provisional-deputy-superintendent-apparition-sub committee of the
Right Venerable the Synod of the Ecclesiastical Courts of the Dutch Reformed Church, Cape Town Cape of Good Hope.

The measurement of the geodetic base line on the Grand Parade, Cape Town, 1837.

proximity to us. These details included (in modern nomenclature) galactic clusters, diffuse nebulae, and planetary nebulae. The drawings which Herschel made of some of the more prominent of these in many cases bear excellent testimony to his powers of observation, now that they can be compared with modern photographs. There are, of course, some surprises. Some of the easily observed objects are misrepresented, but, equally remarkable, some of the most difficult are faithfully represented.

One of Herschel's requirements for this work was the knowledge of the positions of stars which he could use as reference points for the determination of the positions of his newly discovered objects. A considerable proportion of the correspondence between Herschel and Maclear at this time consists of requests from Herschel for the determination, by means of the transit circle or mural circle of the Observatory, of the right ascension and declination of reference stars.

This requirement introduces two themes into the correspondence. One was the errors of the catalog produced by Dunlop at the observatory of Sir Thomas Brisbane at Paramatta, New South Wales. The references are confusing, but the names "Dunlop Catalogue," "Brisbane Catalogue," and others all seem to refer to the same compilation of positions of southern stars. Both men found themselves very critical of Dunlop's work, which they decided was full of systematic and casual errors, causing them a great deal of trouble. Typical of Herschel's remarks are these:

> There are a few (i.e. *not* a *very* few) cases of 12 or 13 sec discord^{n} in these *probably* an error of 10^{s} has been committed on one or other part in the detail of reduction. Where I have examined them your work appears correct. Some cases of 20^{s} and upwards also occur—In the bad cases I have verified my own bringing up of the stars from D's Catal. . . .

and

> On the whole I am sorry to observe that this shewing is even more unfavourable to the Brisbane Catal. than the results of any of my previous comparisons—with other authorities —The errors follow no traceable law. Henceforth I feel disposed to dismiss the epithet the *Brisbane* Catal as connected with this remarkable astronomical record, & rather to designate it by the names of those concerned in its composition— it is worthy of the age of Ulugh Beg or Tycho Brahe. . . .

The award to Dunlop of the Lalande Medal for astronomy by the French Government provokes the remark: "I wish the awarders would come here and look for some of his nebulae and double stars."

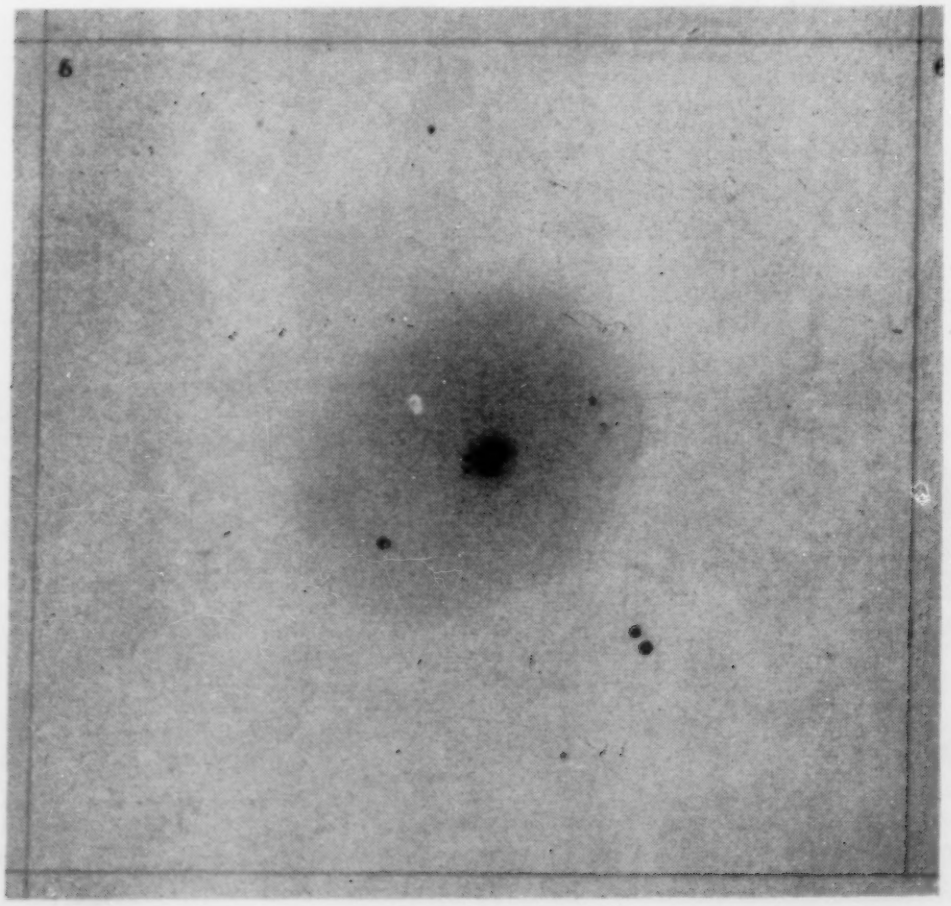
All this is, of course, no more than the customary small change in the expression of mutual esteem between scientific colleagues, but, unfortunately, these two had been rather outspoken in their criticism, and at the end of 1837 Sir George Gibbs, the new Governor of New South Wales, appeared at Cape Town en route to take up his post. He entered the lists in support of the Australians, and Maclear had to eat humble pie, blaming the errors on the fact that the reductions of Dunlop's observations had been carried out in England. This piece of buck-passing did the trick, and peace was restored.

The second topic which united Herschel and Maclear in adversity was the mural circle of the Observatory. One of the principal instruments originally provided was this 6-foot mural circle. This, now obsolete, type of instrument consisted of a telescope pivoted on a horizontal axis, the axis itself being mounted as accurately as possible in an east-west direction by being fixed in a substantial wall running north and south. Thus, the instrument always pointed in the meridian, and the declination of a star observed with its aid could be determined from the inclination of the tube when the star passed through the center of the field. If the time of passage of a star was observed, then, if the right ascension of the star was known, the sidereal time could be measured, and the error of the sidereal clock could be found; or, if the error of the sidereal clock was known, the right ascension of a star could be found by determining the clock time of the transit of the star through the field. In these respects a mural circle closely resembled a transit circle, but, because the mounting of the former was fixed, there was not the possibility of eliminating instrumental errors, as can be done with a transit circle by reversing it in its bearings from time to time.

When the mural circle was being unloaded, in 1826, it was damaged by a fall, and thereafter it never functioned satisfactorily. Fallows had found troublesome errors in the measures obtained with the instrument, and he, Sheepshanks, Airy, and Henderson had all written papers on its behavior before Maclear arrived.

Even today it is never assumed that the art of the instrument maker is so perfect that his products are free from error. Elaborate methods have been devised for making explicit determinations of errors. Herschel seems to have discovered independently many of the modern methods still in use, and his letters include elaborate mathematical discussions of the effects of instrumental errors.

The principal difficulty was the multiplicity of the different kinds of possible error: lack of circularity of the "circle," ellipticity of its pivots, and errors in the graduations were three of them. In their



Herschel's drawing of the nebula NGC5236.

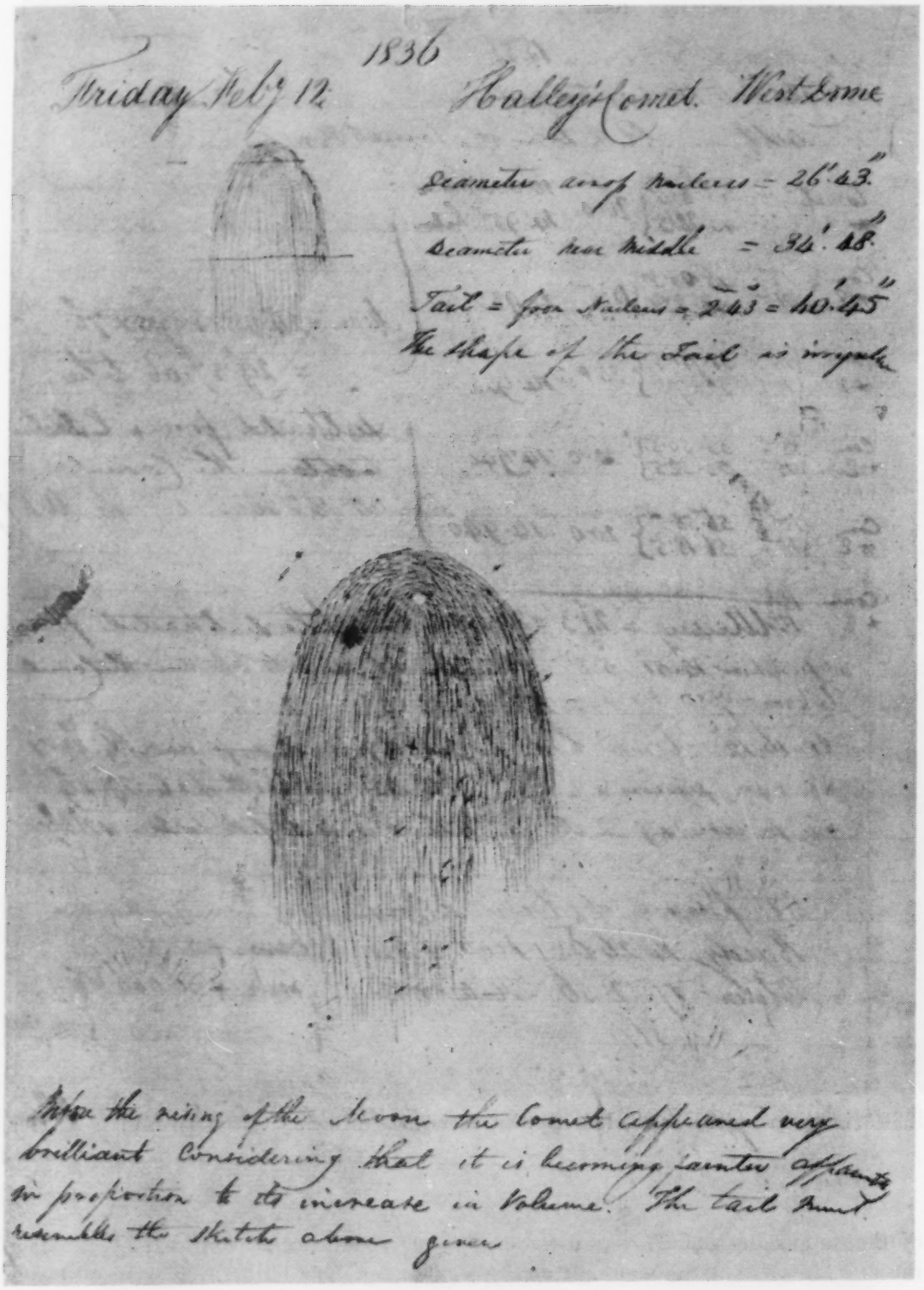
A modern photograph of the nebula NGC5236 (image reversed as compared with Herschel's drawing). [By permission of Her Majesty's Astronomer at the Cape]

efforts to disentangle these, Herschel proposed the use of *Fühlhebeln,* or levers of contact, which would register deviations of form much as the modern dial gauge does; he also proposed to observe the movements of a dot placed on the end of the axis as the instrument was turned. To determine the circle errors he thought of mounting two microscopes on the circle at an interval of ten degrees, and, since the circle and the telescope could be clamped and unclamped and moved relative to each other, to stride round the circle with this "ten degree" interval until a complete circuit had been made; the closure error would then give the true value of the "ten degree" interval, and the graduation errors could be inferred.

The story of the mural circle ends in complete anticlimax. The instrument was never satisfactory and was finally returned to England and stripped for renovation. It was then found that one of the supporting collars was loose on the axis. In view of the fact that nearly a dozen elaborate mathematical discussions of this instrument had been published, there is probably a useful moral to be drawn.

Halley's Comet

One of the great astronomical events to be expected during Herschel's stay was the return of Halley's comet, which was expected to pass through perihelion on 7 November 1835, in the southern sky. Both Herschel and Maclear observed it assiduously, and they originally intended to produce a joint publication containing their results. In the event, this did not come to pass, and Maclear produced an independent publication in the *Memoirs of the Royal Astronomical Society* for 1838, profusely illustrated by drawings made by Piazzi Smyth. The correspondence is confused by the fact that, in their letters, the writers always referred to "the Comet," whereas in fact there were two comets—Halley's comet and Encke's comet, the latter being seen in September. Halley's comet had already been seen in Europe before perihelion and had aroused great excitement. In the south it was eagerly awaited, but there was trouble with the weather, moonlight, and the obstruction of Herschel's western horizon by mountains (which he could not remove) and by trees (which he did). In an undated fragment Herschel wrote, "Somebody told me today the comet had been seen in Cape Town—with Naked Eye—this evening has been so uniformly cloudy as not to give a chance—This is very provoking—as the situation *now* is getting favourable and it ought to have a tail or at least to show a pretty considerable nucleus. But there is a vile moon in the way." Then Maclear found the comet,


Friday Feb 12 1836 Halley's Comet. West Come
Diameter of Nucleus = 26'.43"
Diameter from Middle = 34'.18"
Tail = from Nucleus = 2°43' = 10'.45"
The shape of the Tail is irregular
Before the rising of the Moon the Comet appeared very brilliant Considering that it is becoming fainter apparently in proportion to its increase in Volume. The tail almost resembles the sketch above given

Maclear's drawing of Halley's Comet, February 1836 (from his observing notebook). [By permission of Her Majesty's Astronomer at the Cape]

and turned to Herschel to make sure he had not confused it with a nebula. "I think I may give you the joy of being the first Rediscoverer of Halley's Comet. I find no known nebulae in the places you describe." Herschel was particularly put out by the fact that his mechanic, John Stone, and his son James casually happened to mention that they had seen Halley's comet over Table Mountain one evening with the naked eye and had forgotten to tell him.

On 19 November 1835, there was a total solar eclipse, and Herschel was much excited over the possibilities: "By the way, in this eclipse, which is a *total* one in some parts—the *Comet* will be seen—in or near its perihelion!! Seneca witnessed such a phenomenon in the year AD 60—what a pity the line of total shadow is wasted in the desert of S. Africa and Guinea, But in Madagascar and the Indian Seas it will surely be observed & we shall no doubt hear of it. . . ." This expectation does not seem to have been borne out, but they continued to observe and discuss Halley's comet until March or April of the next year.

Another unprecedented event which Herschel was, this time, privileged to witness was the Nova outburst of the star Eta Argus (Eta Carinae); the nebulosity round this star forms the subject of one of his most detailed plates in his *Observations at the Cape of Good Hope.*

This star had been a relatively inconspicuous "naked-eye" object until it flared up in December 1837; after the outbreak it declined until it is now below naked eye visibility, although still somewhat variable in brightness. Understandably all agog with his discovery, Herschel wrote to Maclear at 10:30 at night on 16 December: "I beg to call your attention to η Argus as a most remarkable variable star. Tonight it is superior to α Eridani and not much if at all inferior to Rigel. . . ." He continues with a rehearsal of his previous observations of the brightness of the star, and resumes: "Tonight it is ridiculous to class η with such stars as Fomalhaut or even with Procyon. He laughs α Crucis to scorn.

"Query how big *will* he grow? It has come on very suddenly. Tonight he caught my eye as *a great star unknown* and I was obliged to shift my place of obs to get the cross in sight before I could be sure it was η.—I have noticed him several times this season at his usual lustre (i.e. not presenting anything remarkable) and it is not above a week or 10 days since I was saying to Stone—'There's η Argus getting high—we must soon begin upon him again'. . . ." At its brightest, η Argus was the fourth brightest star in the sky, but its decline was rapid.

In quoting all these magnitudes Herschel used a method developed by his father. This is the method of sequences, still in use in a modified form, where the magnitude of a variable star or other unknown object is estimated with reference to a sequence of known stars whose brightnesses straddle that of the unknown one.

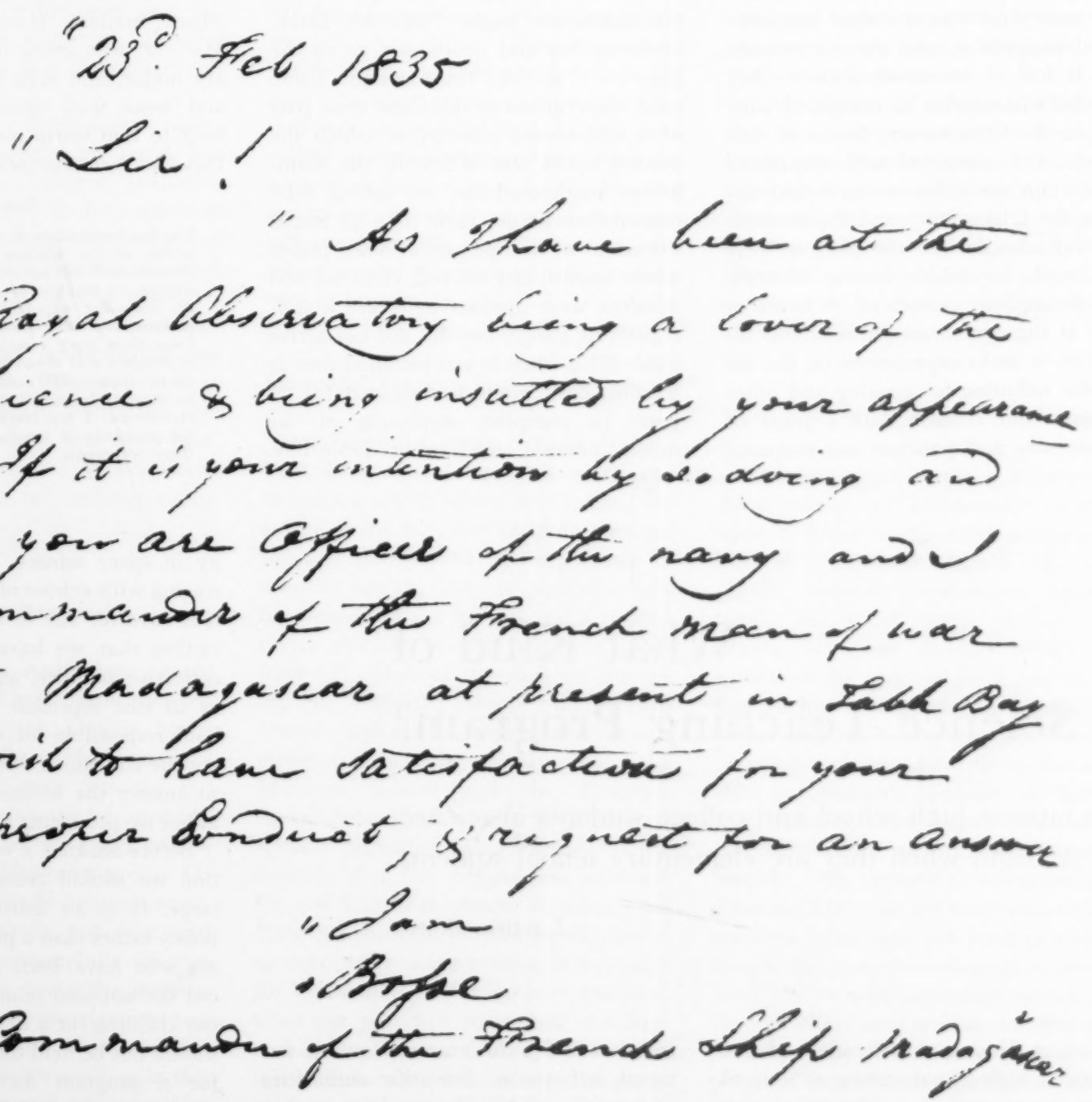
The "Astrometer"

During his stay in South Africa, Herschel developed what seems to have been the first reliable method of actually measuring the magnitudes of stars, an exercise to which he was prompted by the gross and obvious errors in the statements about star magnitudes on the existing charts of the southern sky. This device he called the "astrometer," and the accuracy attainable with it was not to be despised, for some of his results were not bettered until almost as late as the beginning of the photoelectric era. Herschel used a total reflection prism and a lens to form a reduced image of the full moon, the equipment being mounted on a string-operated slide so that the reduced image could be moved

to different distances from the eye of the observer, the apparent brightness thus being made to vary. No telescope was used, and the observer simply held up the device so that he could see both the star to be measured and the little lunar image. He then slid the optical unit to such a distance that the star and the lunar image appeared equally bright, after which he measured the distance from his eye to the artificial star and used the inverse square law to derive a magnitude for the actual star. This simple device is typical of the genius which Herschel had for neat improvisation, and it is regrettable that his use of it seems to have been rather limited.

The flavor of the Herschel correspondence makes it clear that he was well versed in the classics and in the antiquarian lore of the constellations. Naturally, too, from his researches both in the North and the South, Herschel was familiar with all the constellation names then current, and he formed the opinion that reform of the nomenclature and boundaries was long overdue. In his day the northern constellations were much as we know them now, and the principal southern constellations were, and are, as they were named by Lacaille. Herschel embodied his plan for constellation reform in several letters to Maclear. Briefly, he wished to have every constellation of moderate extent bounded by a convex polygon, and to have either star-free vertices or vertices marked by stars. All names were to be different—and not names of individuals but of classes of persons, such as Rex, Heros, Poeta, and so on. All stars brighter than the fourth magnitude were to have individual names, and groups of stars were to be named after "well recognised classical assemblages" such as Pleiades, Muses, Graces, or Nymphs. Another idea was that the initial letter of the name of the constellation should correspond roughly, through its alphabetical position, with the right ascension of the constellation, and (to quote) "that all attempts to accommodate the attitudes or outlines of Figures to the stars of constellations . . . be given up as useless & hopeless labour and that maps with figures on them be regarded as children's toys."

Maclear quite liked the proposition but felt that it would not be accepted, for Herschel displayed the eccentricity of wanting to keep Orion as a constellation but with the figure reversed. That reform was desirable can be seen from some of Herschel's lists: there were Hydra, Hydrus, and Hydra Femella (now obsolete) as well as Serpens and Draco; a Musca in both the North and South, and Apis and Apus adjacent to each other, as well as many other dupli-


"23d. Feb 1835

"Sir!

"As I have been at the
"Royal Observatory being a lover of the
"Sciences & being insulted by your appearance
"If it is your intention by so doing and
"as you are Officer of the navy and I
"Commander of the French man of war
"the Madagascar at present in Table Bay
"I will to have satisfaction for your
"improper Conduct, & request for an answer

"I am
"Sir
"Commander of the French Ship Madagascar

Extract from Maclear's diary quoting a challenge to a duel. A French naval officer visiting the Observatory felt that he had been impolitely treated. In his diary, Maclear described the note as ". . . a literal copy—written on a slip of dirty paper & sealed with a bit of wax with the impression of the thumb!" The duel was not fought. [By permission of Her Majesty's Astronomer at the Cape]

cations and possibilities of confusion. It is regrettable that Herschel conceived his plan in his own exuberant and slightly eccentric fashion and that he does not seem to have pressed the matter further.

Observing the Tides

Two other interests of the pair were meteorology and tidal observations. For Maclear these subjects formed part of his official instructions. For Herschel they were opportunities to exercise his somewhat wayward genius. In addition to the routine work, Herschel invented an instrument called the actinometer, which consisted of a thermometer with a large bulb filled with a dark-colored liquid. This was placed in the sun and the shade alternately, and the rate of rise of the liquid surface gave an opportunity of making rough measurements of the incident intensity of the solar radiation. Both men served on the local meteorological committee, and the correspondence is full of reiterated requests that Herschel's barometer be compared with that at the Observatory. Some of this concern was connected with the desire to ascertain the difference in height between the Observatory and Feldhausen. Herschel also devised the plan, already mentioned, for taking hourly observations throughout periods of 24 hours or more at the equinoxes and solstices. He went on to make experiments on the use of solar radiation for cooking and other purposes and records that a meal of mutton stew and potatoes was prepared in his solar cooker. His best systematic study was the detection of an annual fluctuation, and a daily fluctuation the magnitude of which varied with the seasons, in the barometric height.

The investigation of the tides was another subject which fell within the scope of Maclear's official instructions, and Maclear began the installation of a tide gauge in Table Bay in 1835. Thereafter this topic took an increasingly large share of his attention, for he took an interest not only in the tides in Table Bay but in those in Simon's Bay as well, and the apparatus employed became steadily more complicated. In the early days, Herschel, "assisted" by Bowler, would take the readings himself. Later on, a man who rejoiced in the profession of "tidewaiter" was employed, and Herschel devoted much time and thought to the problem of devising an apparatus to ring a bell when the tide was at midwater (either rising or falling) so that the tidewaiter might "take his cloak, lanthorn, key and pencil and go to the gage . . ." to take the readings. These tidal observations at the Cape were part of a widespread scheme, of which the moving spirit was Whewell, the Cambridge mathematician, for taking tidal observations at the same time all round the coasts of Europe and at other places where opportunity offered. Herschel and Maclear were anxious to secure a self-registering gauge, but this did not arrive until 1840, when it was installed first at Simon's Bay, where it remained for 20 years (a complete revolution of the moon's nodes), and later at Table Bay.

Herschel occupied hi[illegible]lf to a great extent in the statistical [illegible]d harmonic analysis of the results, d[illegible]nstrating the presence of the terms d[illegible] to change in the lunar and solar decli[illegible]tions, but the subject soon became a r[illegible]tine one, and Herschel turned to more [illegible]ried fields.

Finally, in 1838, Hers[illegible]el packed up his instruments, sold his [illegible]ouse, and returned to England. It had been a lively time in South Africa, with many new interests and stimulating problems. For Herschel there was a dignified and unproductive middle age, full of honor and respect. For Maclear there was the memory of four years of moral support and intellectual stimulation, and, for him, there were many years more of productive scientific work. The memory of their time together remained with them both. Maclear, having ceded his pension to his wife, was financially embarrassed when she died before he did, and Herschel intervened in official quarters to adjust matters. Herschel died before Maclear, who wrote the former's obituary notice, and even then, though aged and beset with blindness, there came back to him warm memories of all that they had done together (*1*).

Note

1. Use has been made in the preparation of this article of the Maclear papers in the South African Archives, particularly the file of letters written to Maclear by Herschel. In addition, the following unpublished documents in the possession of the Royal Observatory at the Cape have been consulted: manuscript diary of Maclear and observing notebook of Maclear (both about 1835); correspondence referring to Bowler; fragments of the diary of Thomas Henderson. I am much indebted to my wife for assistance in the transcription of many of these documents.

What Kind of Science Teaching Program?

To interest high school and college students in science, begin when they are elementary school students.

Laetitia Bolton

Herbert Hoover's indictment of the American high-school system as "one of the major causes" of our failure to produce enough scientists and engineers is typical of one kind of widespread public reaction to Sputnik's assault on our national self-esteem. Scientific authorities and politicians have been telling us that we should not have been surprised by this revelation of the Russians' superiority in space science. In an atmosphere ringing with echoes of "We told you so," it is hard for any of us to escape a conviction that we have made a massive, collective blunder, and it is natural for us to cast reproach on the people we hold responsible for it. First on the list are the educators, who are now hard put to answer the accusing question, "Why aren't we producing more scientists?"

Before making a scapegoat of education we should recognize that, by and large, it is an instrument of national policy rather than a prime mover. Teachers who have been struggling to carry out the national mandate to educate all our children for a democratic way of life should not be held too strictly to account for a program dictated primarily by school boards, which are more or less

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responsi[illegible] students and their parents, and by t[illegible]ational economy.

Why Is S[illegible]ence Unpopular?

Of course it is shocking that only about one-fourth of all our high-school students study physics, only a third take chemistry or geometry, and only two-fifths take intermediate algebra. Yet we cannot agree with Mr. Hoover that the failure of students to go in for science or mathematics stems primarily from their wish to escape hard work. Science is unpopular partly because of cultural pressures, because of our heritage of anti-intellectualism, which makes young people suspect that a scientist is a queer duck, not quite human and probably not very happy. Science also fails to attract many students for the simple reason that they have had little opportunity to become interested in the subject. At the elementary level, for the most part, science is either not taught at all or is presented in a way that discourages enthusiasm.

Children who are exposed to a stimulating science experience are much less subject to adverse cultural pressures. They are not so likely to be frightened by what they hear about the destructive aspects of science, and they are less awed by popular misconceptions of what scientists are like. Those who have experienced personally the joys of search and discovery that are the scientist's rewards are far better able to appreciate his achievements and enter into his world. Because they have found science exciting, they are eager to continue studying it.

Should We Copy the Russians?

Most intelligent Americans would agree with Wernher von Braun that science education in the United States needs the kind of "powerful boost" he hopes that Sputnik may provide. But where should the pressure be applied? Is more required science in high school the answer, as Mr. Hoover suggests? Or should we adopt a crash program modeled on Russian methods, impose a rigid "ten-year schedule" on carefully selected children in order to assure the production of an aristocracy of scientists and technologists? Responsible American educators, and most scientists working in this country, do not believe that this kind of program would be either workable or desirable for us. Whatever course we adopt must be contained within the democratic structure of our society.

Moreover, the kind of science teaching we provide must be different from the Russians', and differently motivated, because our over-all aims are not the same. Here is where the educators come in. It is their responsibility not merely to train more scientists but to foster understanding of science. They must undertake to create more enlightened citizens, capable of integrating knowledge and appreciation of scientific aims and achievements into our general culture. As Edward Teller said in his appearance on "See It Now" last December, "we need more science fans." James R. Killian has pointed out that "the liberal arts of our time cannot be liberal if they reject or disdain science and technology," and that, conversely, "science and technology cannot fulfill their responsibilities if our scientists and engineers lack the humanistic quality which has been ascribed to the Athenians—the art of making gentle the life of mankind." This integration, which should be basic to our whole science education program, cannot be carried out successfully if it is left until college or even high-school age. We must start by making science intelligible and interesting to children while they are still young enough to incorporate it into their scheme of values as well as their core of knowledge.

Successful Teaching Produces Results

The Ethical Culture Schools of New York City, where science is introduced in nursery school, afford an excellent example of the fact that children who enjoy a satisfactory science experience at an early age are likely to acquire a lasting interest in the subject and a sympathetic attitude towards scientific problems (*1*). Most of the students attending the Fieldston School—the Ethical Culture high school—come from the two elementary schools in this group, the Midtown School and the Fieldston Lower School, which offer well-planned science programs. The science faculty believes that this preparation accounts for the fact that about 70 percent of Fieldston students, boys and girls, choose to take more science than is required for graduation or for college entrance. Over the past five years, half the boys and about 6 percent of the girls have taken all three sciences—chemistry, physics, and biology—offered in the upper school. These subjects are elected by the students from a rich curriculum, which includes classical and modern languages, many arts courses, and a full athletic program for all students, in addition to other basic subjects commonly offered in high schools (*2*).

About one-fifth of all Fieldston students choose an accelerated mathematics program leading to a year of college mathematics in their senior year. A high proportion of graduates also qualify for advanced placement in college chemistry and physics. This outstanding record is not due to pushing or cramming. Every effort is made to avoid overloading students or increasing tensions caused by anxiety over college admissions. For each student the science or mathematics course is kept to what he can absorb and enjoy.

Conservation, also introduced in the elementary schools, is taught at the high-school level as an integral part of social studies and biology. An extensive, laboratory-based science program in the seventh and eighth grades makes unnecessary the conventional ninth-grade "general science" course.

This interest is reflected in the fact that, in a recent survey of Fieldston graduates, more than a fourth of the careers reported were in the fields of science, medicine, or science teaching. This is a significant number for a school which offers a general college preparatory course with no special emphasis on any one area of learning.

What Kind of Program?

To produce this degree of voluntary involvement, an elementary program must be based on the premise that every child is a potential scientist. The small child's natural, unbounded curiosity about his world—about animals and plants, wind and weather, sunrise and sunset—is the most powerful tool he and his teacher can use to investigate this world and its forces. Anyone who has watched children absorbed by the wonders of Chicago's Museum of Science and Industry, New York's Museum of Natural History, the Hayden Planetarium, or "Mr. Wizard's" television program on NBC, can see with what delight children latch onto this kind of presentation. But their introduction to science need not be even so formal as this and is more effective if it is an integral part of their daily life at school. A science program for children should grow out of their constantly expanding interests, should satisfy their curiosity and stimulate it to further discovery.

How Young Can They Start?

It is interesting to note that even advocates of elementary science education do not always realize at what an early age science can become a part of children's lives. Senator Margaret Chase Smith, in a recent plea for teaching science in grade school, said: "Obviously we can't start teaching science to 6- and 7-year-olds in the first grade." Yet schools which gear their teaching to the experience and interests of children themselves find that even in nursery school children can acquire some understanding of and appreciation for science. At the Fieldston Lower School, for example, last winter's first snowfall triggered off a month-long investigation of the effects of temperature by a group of five-year-olds who wanted to know why the snow melted when they brought it indoors. They tried refreezing it and went on to find out something about how snow differs from ice and how refrigerators work. To discover some of the effects of heat, they baked cakes in an oven, checking time and temperature. This is a typical example of how an imaginative teacher can utilize the common experiences of everyday life to introduce young children to the joys of scientific discovery and the fundamentals of research.

Last February, Governor Harriman signed a bill making science teaching mandatory in New York State from the first grade through the eighth, beginning in September 1958. The state's recommended curriculum, already adopted by many New York elementary schools, is built around ten topics: "kinds of living things, keeping healthy, using electricity, common chemical and physical changes, lifting and moving things, energy from the sun, the atmosphere, earth and sky, rocks and soil, survival of living things."

This is an interesting list which should prove suggestive to teachers, but much of the success of the new program will depend on how these topics are approached. Cornelius Denslow, science teacher at the Midtown School, has pointed out, for example, that "information per se is not understood or retained unless the child is ready to receive it. Only when it satisfies a genuine curiosity is it truly assimilated" (*3*). Let us hope that in our zeal for science education we do not make the mistake of trying to "cover" prescribed subject matter at the cost of discouraging real interest in young people. "Science is not a mere matter of information," says Randolph Smith, director of the Little Red School House in New York City. "Keeping children's natural curiosities alive and fostering the essence of the scientist's avid search for knowing in every nook and cranny of his world is the nub of the job." There are all sorts of ways in which this can be done. At the Little Red School House, as at the Ethical Culture Schools, the earliest science experience is often focused on the care of animals and their young. A rabbit, a snake, a mouse, or a skunk may be not only a treasured classroom pet but also, for city children especially, an open-sesame to a new world of living things. This leads by degrees into a study of biology and human physiology.

A seven-year-old group at the City and Country School, another independent elementary school in New York City, recently did some research on hydraulics in order to install a water system for their building-block reconstruction of Manhattan. When an over-enthusiastic "engineer" poured too much water in the sand-pile reservoir, the class began investigating the effects of soil erosion.

At the Little Red School House, a dying jack-o'-lantern that developed mold once inspired a month-long exploration of molds (including penicillin)—where they come from, how they grow, and what they are good for.

Science experiences often grow out of other classroom work or school activities. The eight-year-olds who run the school post office at the City and Country School recently solved a complicated electrical problem when they found that the "special delivery bell" they had installed, to be operated from the school office, did not work. With the help of the science teacher they did some careful testing and found that the steel wire they had used provided too much resistance to carry electricity from their small battery source. In copper wire they found the answer to their problem.

With no sacrifice of its importance, science may be closely integrated with social studies. For example, the nine-year-olds who run the school supply store at the City and Country School study the products they sell. This usually involves a visit to a paper factory, with an introduction to chemistry through the process of making their own paper in the science laboratory.

The city itself becomes a laboratory as children investigate how fires are extinguished and how garbage is disposed of. In the course of constructing and lighting a model of their city, students at the Little Red School House discover the underworld of subways, cables, and conduits that lie beneath the streets. They find out how power is produced, how pumps work, how gravity and friction affect the operation of all these utilities.

An unusual workshop-laboratory such as that which the Walden School maintains for nine-, ten-, eleven-, and twelve-year-olds may help to provide, for city children, a substitute for the kind of opportunity offered by a rural attic or suburban junk-pile—a chance to take things apart and put them together; to handle, feel, discover for themselves how things work. From old radio parts, old telephones, clocks, or vacuum cleaners these youngsters sometimes derive as much scientific understanding as from the more formal laboratory equipment which the school also supplies. One boy, for example, made out of three old radio rheostats an apparatus to regulate the lighting for a school play. An old motor was repaired and used to spin colored disks, which led to a study of light and color by children of different age groups working together.

In all these schools, photography serves as an exciting introduction to chemistry and physics, as children learn to develop and print their own pictures. A child who is fascinated by glass blowing may go on to make his own thermometer or hourglass. At the Midtown School, more formal instruction may include carefully supervised chemistry experiments—making ink, freeing oxygen, crystallizing chemicals from solutions, separating mercury from mercurous oxide. At Walden, children do some dissecting of earthworms, crayfish, frogs, fish, and chickens. An eighth-grade study of ecology climaxes the science course at the City and Country School.

Technique and Equipment

In all these schools children become familiar with equipment and learn to handle it efficiently and safely. All except the Little Red School House have a full-time science teacher and a well-equipped science room which is used by the older children for classes and "free period" science work. These are important assets, but the equipment for an elementary school laboratory need not be elaborate or costly.

Techniques, Denslow feels, are best taught in response to "children's natural desire to learn better ways of doing things." Katharine Reichenbach of the

Walden School finds that techniques often impose when a child becomes so fascinated by an experiment that he does it over and over again.

Must We Scrap Democracy?

I have suggested only a few of the ways in which early science experience can be made a vital part of every child's education. I hope that these examples may suggest the kind of elementary science teaching which should be an important part of our answer to sputnik. One of the immediate reactions to its launching was to lay the blame for our tardiness on democratic society. "Too much gabbing has been going on these days," Max Ascoli wrote in *The Reporter* last fall, "about the prospects—if not, indeed, the actual evidence—of the superior capacity a slave society has over a free one in getting things done." John R. Dunning, dean of Columbia University's School of Engineering, reminded us in an excellent article in the *New York Times Magazine* that "we should not be deluded into thinking that dictatorship is necessarily more efficient than liberty. . . . The voluntary principle is the very thing we are defending in the cold war."

If we reject, as most of us do, the notion of drafting scientists, we must find some better means of increasing our supply. Most of the current clamor for reform has been aimed at the upper levels of education. But high schools complain that students are not interested in science; colleges find them ill prepared. The widespread introduction in American schools of science teaching, democratically motivated by the interest and curiosity of younger children, would seem a made-to-order method for raising the level not only of our science education but of our culture as well.

Notes

1. An issue of the Ethical Culture Schools' publication, *School and Home*, for March 1930 reveals that at that time science had long been an important subject at all levels in these schools.
2. The students' choice does not appear to be motivated by parental interests, since Fieldston families represent a wide range of occupations and cultural backgrounds. Since a majority of the students are admitted in the primary or preprimary grades, where no intelligence tests are made, the factor of exceptionally high IQ ratings does not apply.
3. *Sci. Teacher* (December 1956).

Popularizing Science

Can it be done? One opinion is that very little of what scientists know can ever reach the public.

M. W. Thistle

"It is often said that the presentation of scientific results to the general public in assimilable form is an important task for our age, and so it is; but the best way of doing it is perhaps yet to be found."—Charles E. Whitmore [*Sci. Monthly* **71**, 337 (Nov. 1950)].

What scientists do, it seems to me, is to try to find out what is going on—either inside us, or outside us, or both. Scientists, then, are definitely set to eradicate mystery. Yet nonscientists, including some members of the popular press, tend to believe that there is something mysterious about science; that in consequence it is appropriate to regard scientists with awe, to praise them unmercifully, to laugh at them, to be afraid of them, or to consult them on questions that are not always framed in clear and answerable terms.

Nonscientists tend to believe that a scientific institution is swarming with eye-popping discoveries every Tuesday, most of which the scientists conceal because they are overcautious. Laymen cannot bring themselves to believe that most science is singularly undramatic; it is difficult to convince them that science is not a continuing series of spectacular advances, on all fronts at once.

Scientists, on the other hand, are very much aware that hundreds of scientists and thousands of engineers worked for a significant fraction of a century on the problem of nuclear fission. They feel that laymen must learn not to expect the same sort of spectacular success every week, in time for the rotogravure section; must learn that the progress of science is a slow creep, consisting of thousands of small successes; must learn that only now and then is a result achieved that is both dramatic and obviously meaningful to the multitude.

How did this situation come about? How is it that men who patiently try to remove as much mystery as possible, no matter how long it may take, are regarded as mysterious figures, crackling with sudden and frequent revelations of further mystery? Can it be that the task of talking about science to a lay audience is particularly difficult?

I have arranged a diagram of barriers to illustrate the situation (Fig. 1). The first barrier is a single one—man directing questions to the universe—but all of the other barriers are double: barriers inside the communicator plus barriers inside the communicatee.

I shall discuss first the possibility of giving detailed and accurate accounts and then the possibility of transmitting scientific attitudes.

First Barrier: Words versus Things

Here the problem is to record new information about what is going on in the universe, somehow in terms of human symbols. This relation between things-going-on and human symbology has fascinated me for years. In the present context, it is enough to note that this problem does exist and to remark that even the wisest words serve as rather poor maps of what is really going on.

Second Barrier: Language and Sophistication

Everyone is aware of the language barrier; most people are also aware that some few persons are much better than others at talking about science to laymen, and so they pin their hopes on these unusual people and trust that all will be well.

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However, even when you have found someone who is very good at talking about science to laymen—and a rare breed it is—all is by no means well. Other barriers exist that make the language barrier seem quite trivial by comparison. I shall review the language problem very briefly and then get on to consider stages of scientific sophistication.

Language barriers. For many purposes, English is a flexible and beautiful thing; I have been drunk on it many times, when it was used by someone who knew how to use it. But if English—or French or Chinese—had been suitable for science, we would not have been forced to invent a number of better languages *for scientific purposes.* For this job, English is hopelessly inadequate. Some of the scientific languages, such as that of organic chemistry, have more than twice as many terms as English (are twice as "rich") and are structurally superior in a quite dramatic fashion.

To ask a man to translate from one or several rich, relatively new, and precise scientific languages into a single poverty-stricken language of inadequate structure—with the built-in faulty science and outmoded thinking of previous centuries showing at every seam—is asking a very great deal. Whatever detail this man does manage to get across to a general audience will certainly be distorted and, to some extent, actually false. No other outcome is possible.

If you listen closely to a man who is supposed to be good at talking about science in English, you will notice that he is not trying to transmit many details—he is giving broad outlines, general trends, and a highly condensed abstract of results. If he really is good, he knows that scientific details cannot be transmitted in an undamaged condition to a lay audience, or even to a nonspecialist scientific audience.

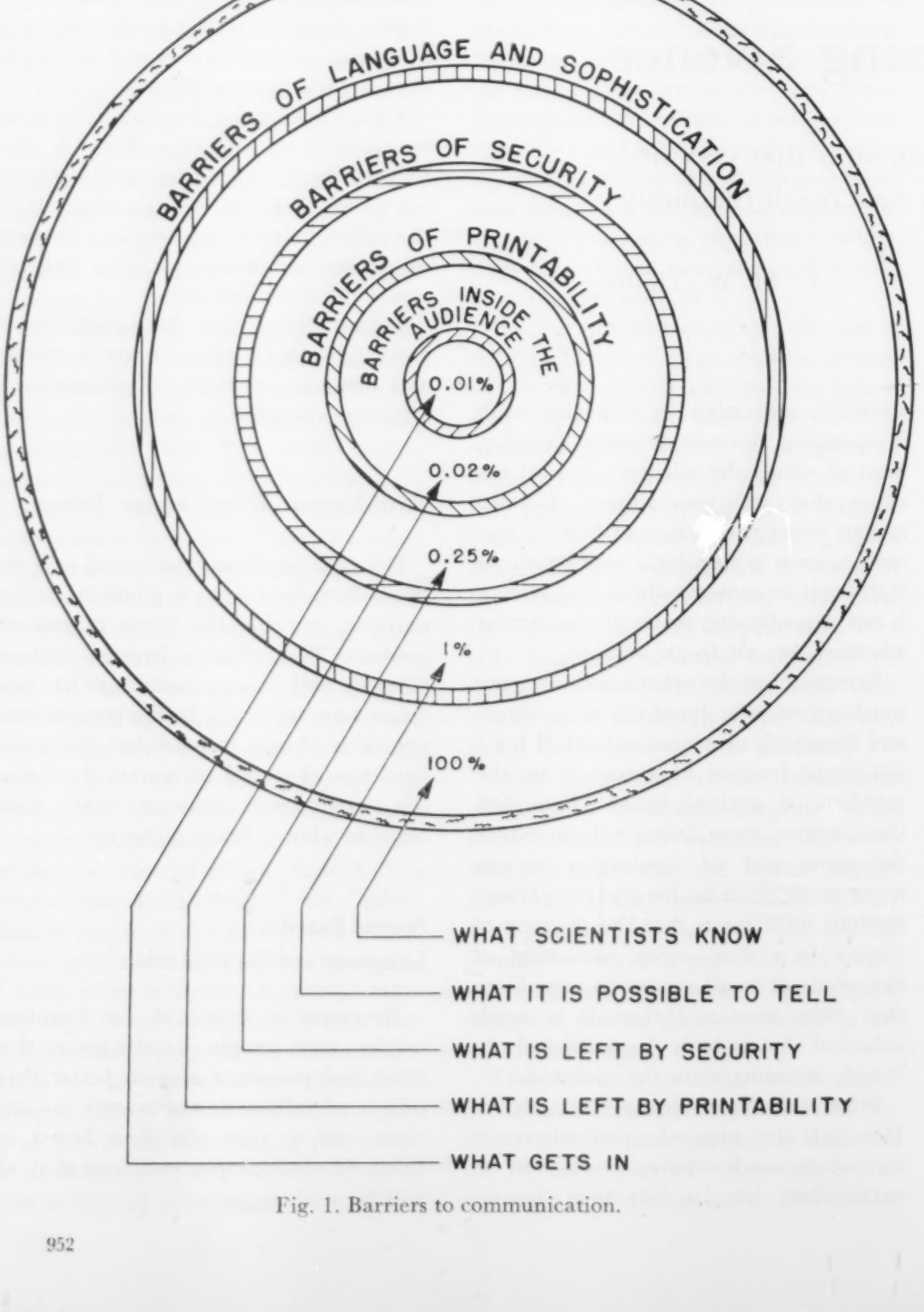

Fig. 1. Barriers to communication.

Laymen think it is quite natural for them to be ignorant of the scientific languages; it strikes them as unnatural when a scientist is ignorant of English. But as a matter of observation, only a very small percentage of *either* group is able to operate competently in the English language. Competent literacy is quite rare, and it is unfair to expect every scientist to be also a master of the common tongue. It would be astonishing if every scientist—or every butcher—were also an author in his spare time.

Rightly or wrongly, the language barrier is generally felt to be inside the scientist rather than inside his audience, and there is some justification for this view. I know a lot of scientists whom I love, but whose operations in the English tongue remind me of an elephant on stilts—ponderously inelegant. However, before we become too scornful of the scientist who is clumsy in English, let us have a good sharp look at the audience.

Stages of scientific sophistication. Gaston Bachelard, professor of the history and philosophy of science at the Sorbonne, in his book called *La Philosophie du Non,* gives a very useful listing of the five stages of scientific sophistication (Fig. 2):

1) *Primitive realism* might be stated, very briefly, like this: $x + y + god = 0$. If you are trying to understand something, but can't, you can blame it on the whim of a god, or perhaps on the work of a devil.

2) *Empiricism* has at least advanced to the notion of using measurements made with rulers, thermometers, and so on. Then you can say how hot it is without referring to Hell.

3) *Classical science,* typified by Newton, has advanced to the notion of using laws: the law of gravity and that of the relationship between temperature and pressure, for example.

4) *Modern science,* based on Einstein, has advanced to the notion of conversion: matter into energy, and so on.

5) Even Bachelard admits that *advancing science* is difficult to define. So far as I am concerned, advancing science, based on the notions of men like Dirac, tells us that we cannot even

imagine what an electron is like, but must use mathematics. It is the end of any sort of comfortable representation of "reality" but compensates with free-wheeling worlds of 'if'." It closes certain things to our imagination and, at the same time, invites us to make much more use of our imagination. Confusing, isn't it?

Before we go any further, let us get rid of any idea that *value* is attached to these stages. None is to be despised. Any one of them is rich in its own right and *different* from the others, but it is *not* better than the others. Some of my dearest friends are almost pure "primitives" (stage 1 on this scale); the only book I have published is almost pure "stage 1," and I am not ashamed of it; I wish I were a better primitive than I am, and I have no burning desire to be a stage-5 man—I'm having fun where I am, mostly in the first three stages. Let me say it over again: these stages are *different*; they are *not* like certain soaps, better, period.

To me, the most important part of Fig. 2 consists of the upright bars between the various stages. It suddenly occurred to me that these barriers are real. To go from one stage to the next—through the barrier—takes a tremendous amount of effort and many years of hard study. That is why so few of us ever get into stage 5.

Recently we had a stage-5 man visit the National Research Council of Canada—Dirac himself. Dozens of stage-4 scientists stayed away from the lectures because they would have found them meaningless. For this man to try to tell *me* what he is doing (I am mostly stage 3) would have been like talking philosophy to a monkey. The monkey, of course, could never get onto this scale at all, and it is theoretically possible for me to reach stage 5 in about a quarter of a century of single-minded effort. But right now the comparison is just. Dirac is two barriers away from me (I used to be a scientist myself) and three or four barriers away from most laymen. The chance of his being able to communicate very much through all these barriers is not very great.

So, the language barrier inside the scientist makes communication of *detail* extremely hazardous; but the barriers inside the lay audience—these difficult barriers between the various stages of scientific sophistication—make the communication of modern scientific detail utterly and completely impossible.

It is all very well to look to the past and say, "Whathehell, Darwin did it."

Primitive realism	Empiricism	Classical science	Modern science	Advancing science
1	2	3	4	5

Fig. 2. Stages of scientific sophistication.

But Darwin was a stage-3 man himself, and he wrote for a stage-3 audience. Communication at this level is now within the scope of our high schools. We have added two more stages since Darwin—which is to say that we have added two more barriers.

You may remember a time when only a dozen men in the world understood Einstein, and all of them were in process of going through that barrier themselves. Einstein tried—and in my opinion, failed—to transmit stage 4 downward. I am stage 3, and I gave Einstein's "popular" writings a good honest try. I admit that I now know very little about the details of Einstein's theories, other than to parrot $E = mc^2$.

Concerning details. Very little detail can be transmitted back through even a single one of these barriers; through more than one, the task is hopeless. It is about time that we stopped expecting miracles of even the best exposition; you can get the same glassy stare by declaiming Gaelic poetry to a deaf sea gull. In this you would come up against four barriers: an unusual form (poetry); an unusual language (Gaelic); a defective transmitting mechanism (impaired hearing apparatus); and receiving apparatus so inappropriately trained for the job that it might just as well not exist (the cortex of a sea gull). So much for our chance of communicating scientific details.

Can we then say nothing? On the contrary, we can still say a great deal. One way of reaching down past the barriers is to find something familiar to everyone and talk about that. When Ray Lemieux synthesized an obscure carbohydrate called trehalose, the world of chemistry was jubilant, but the public did not care. However, a few days later he synthesized sucrose—the sugar you put in your tea—and now you have a story.

This is the old rule of writing: to go from the known to the unknown. The trouble is to find something that laymen think they know—something to hitch your story to. Wilder Penfield can talk about brain surgery to a fascinated audience because he hitches what he has to say to, for instance, the problem of bringing up children in a foreign language. You see, people generally have the illusion that they know something about children. Similarly, the bang of an atom bomb will make you listen to a description of isotopes.

When this point of common interest has been found, we have some hope of transmitting broad trends, general conclusions, and a few carefully selected results.

Third Barrier: Security

The barriers of language and sophistication obstruct communication of all but a very small amount of what the scientist knows—of all but perhaps about 1 percent. The third barrier, "security," obstructs communication of, at a rough guess, about three-quarters of what is left. Security exists in several forms. Military security accounts for a good many restrictions, but so does economic security: it is most unwise to reveal certain developments until after they are patented, and, beyond that, it is still unwise to reveal these developments before they have been actually adopted by an industry. Otherwise we are plagued by people who want to buy the thing: the general public confuses invention with business. Political security is also encountered.

Since all forms of security tend to involve applied science only, it follows that security attaches only to those items that are relatively easy to talk about; hence the high mortality.

Fourth Barrier: Printability

Items that survive the first three barriers have to compete for space in the public press against all other stories from all other fields of human activity. This means that the scientific story had better be important, and it had better be well written. Even then, a local fire or a major political, economic, or crime story can easily displace the scientific item.

Figures here are bound to be even more shaky than those that have already been given. I use 10 percent as the figure for survival across the barrier of printability because, if I send a story to

400 possible outlets, my average take is about 40 clippings. Of course it would be easy to choose 50 possible outlets and thus change the outcome. But 10 percent seems reasonable to me, in this context. In most of the 40 clippings the story has been cut to the bone, and beyond.

Incidentally, when reporters write their own stories, this figure is likely to be reversed. Their stories occasionally suffer from cutting or displacement but stand a much better chance of appearing in print than does a "government handout," coming through the mail. So it is wise to assist reporters to do their own stories.

Fifth Barrier: What Gets In

It has been stated that the "average" newspaper reader will read one-fifth of the news stories in his paper. Against this we can set the fact that "several" people read each copy sold. But suppose we have finally captured a reader for one of our science stories. What is going on inside him?

Remember that, if I wrote the story, some of my own stage-3 language is probably still there, and the reader has his own barriers, with which it is quite probable that I have not dealt very expertly.

Some readers will no doubt understand every word; others may only be sitting there, performing eye-swivelling exercises. So, on the average, we can guess that about 50 percent of the story actually reaches home in the brain of the reader.

When we calculate what gets through all five barriers, it turns out that this is of the order of one ten-thousandth part of what the scientists know.

It may be only a ten-thousandth part of what scientists know, but this is still a fair amount; after all, scientists deal with information, and they have a tidy stock—about 60 million pages per year at the current rate of production.

However, it may be that the most important thing to transmit is something of scientific attitudes (sometimes called "the philosophy of science"), so we will now examine a few of these attitudes.

Scientific Attitudes

While scientists have managed to retain the sense of wonder and the intense curiosity of very young people, in other respects they have advanced to a maturity of outlook that might be valuable in other parts of our society.

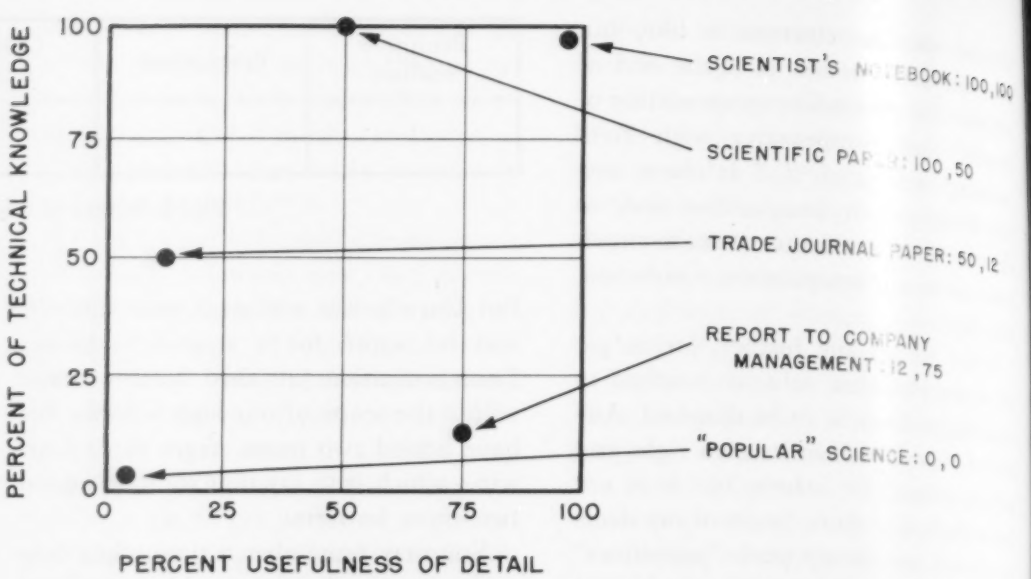

Fig. 3. Graph devised to help a scientist suit his material to his audience. One hundred percent of technical knowledge is, of course, 100 percent of what the scientist has to offer. No great accuracy is claimed for the points—they merely indicate the general area for each audience. [J. A. Anderson]

Maturity of the scientific outlook. Maturity can be defined by what it is not: a mature person, in the sense I have in mind, is not dogmatic, is not damnatory, is not proud. A scientist does not say: "This is what I have found, and if you do not believe me, I will burn your body, and if you escape me, I will burn whatever survives of you in the hereafter." A scientist knows that the mistakes of Aristotle were replaced by the mistakes of Newton, which were replaced by the mistakes of Einstein. He is not interested in being "right"—only in finding out what is going on. To this end he invites criticism. And he really does not care in the least if the other person has a different color of skin, a different color of religion, or a different color of politics. All he cares about is the repeatability of the other fellow's results and any new light that is shed on the universe. Many scientists practice tolerance in their daily lives to a somewhat higher degree than is the case in most other professions.

To judge from what I see in the newspapers, this sort of outlook might well be useful to society in general.

In times past, the scientists' easy attitude with respect to "foreign" religions made the general public a bit uneasy and distrustful; people would have been more comfortable if their scientists had been a bit more intolerant. These days few people seem to care about religious tolerance, but they would be more comfortable if the scientist were more intolerant towards "odd" brands of politics. The fact that they have no cause for worry on this score is shown by the record of our scientists in World War II. A good many of them spent the war years in the front lines, testing their devices. The loyalty record of scientists compares favorably with that of any other large group. They are tolerant, but they are not stupid. Like any other intelligent group, they think that war is folly, but once it starts, you will find them lined up on their own side.

Scientists and the humanities. Every now and then you can still hear someone remark that our scientists ought to come out of their ivory towers, mingle with the populace, and become "more human." It has evidently escaped these folk that scientists *have* done just that, quietly and without any fuss, during the past decade or so.

In their communities, support for the arts and humanities now comes in generous measure from the scientists. You will find a large number of engineers, chemists, and so forth in the audiences of folk singers, ballet groups, local and imported theatre groups, and so on, and likely as not you will find some of them performing in semiprofessional symphony orchestras or, for that matter, even in amateur ballet. Some of them write fiction (you may recall a Canadian best-seller called *Sarah Binks,* written by a chemist); others paint; others take part in local government. In short, modern scientists make excellent citizens. The old idea that scientists are so deeply interested in their work that they invariably neglect their duty to the community is passé. In Canada, the arts and humanities have few better friends outside their own ranks than the scientists.

New responsibilities for scientists. Since science is becoming increasingly important in our daily lives and now touches us so nearly on all sides, the

need for talk about science is on the increase. Scientists can expect to be subjected to increasing pressure to do some of the talking themselves. A great many of them are using public money, and it is not unreasonable to expect them to make an occasional "report to management."

It pains me to say so, but relatively few scientists are yet aware of the enormous importance of reasonable competence in their use of English. If they examine the five steps of the generalized scientific method, they will see that only one of these steps—making the actual measurements—does not involve the use of symbols. And this is the only step that they can leave to a technician; the *scientists* must frame the question and draw the theoretical conclusions. Language is their business.

Aside from their need of reasonably adequate English as an essential part of their written and spoken reports to one another, they will need English for communicating with laymen at various levels —for example, in the trade journal article, the "popular" speech, and the interview with the press. Some of them do these things very well indeed; but far too many of them do these things rather badly.

The ones who do it badly err on two counts: a bumbling, fumbling use of the language itself and a thoroughly mistaken idea of how much detail is required. There is no substitute for adequate training in writing and speaking, but it may be possible to give a quick insight into the amount of detail needed. The graph in Fig. 3, adapted from an original by J. Ansel Anderson, chief chemist of the Grain Research Laboratory in Winnipeg, may be helpful.

At the time this graph was prepared, neither Anderson nor I had heard of Gaston Bachelard. Nevertheless, we agreed that science for laymen was in the area of 0,0: zero technical knowledge and zero detail. Scientists please note.

Conclusions concerning attitudes. Some common attitudes of our scientists bother me, such as their persistent refusal to learn the techniques of communication —resulting, on the one hand, in some of the worst-written documents in the world and, on the other, in a firm belief that the press is out to misquote them deliberately.

Certain other attitudes of our scientists seem to me to be worth pondering: the desire to find out what is really going on, instead of being content with what other people say is going on; the determination to take no man's word, not even your own, for a material fact unless you can put it to the test and observe for yourself; the confident expectation that whatever you do will soon become outmoded and surpassed, and that this does not matter in the least; the expectation that other people will be markedly different from you, and that this is an excellent arrangement; the convictions that doing your best to think straight is a worthy occupation for a full-grown man, that this is a strange and wonderful universe whose ultimate secrets we will never quite plumb, that it is nevertheless the best sport in the world to try to plumb them, that you *never* get something for nothing, that you *always* get less than you expect—and that *never* and *always* are very dangerous words.

One way to spread these notions is for the scientists themselves to do a bit more talking; the attitudes will soon become apparent, no matter what the scientist is actually saying.

In conclusion, I might point out that we have some very old precedents for breaking through the barriers and talking to ordinary folk about extraordinary things. Jesus had such a problem. His technique was to put what he had to say into a perfect little short story, dealing only with familiar things that you can touch and see. He would begin with, "A certain man had two sons," or "Behold, a sower went forth to sow." To this day, the only device I know that will actually work for an audience of fishermen, taxgatherers, publicans, housewives, or other groups of laymen is this same technique of analogy, comparison, metaphor, simile, and parable.

Making Popular Science More Popular

Tested techniques of communication can help make the leading ideas of science clear to the layman.

John Pfeiffer

It is a pleasure to comment on M. W. Thistle's thoughtful and stimulating article, which comes as an important contribution to the continuing problem of bringing science to nonscientists. Two attitudes may stand in the way of those most actively concerned with communication in this area. There are still scientists who feel that the problem cannot be solved, that the layman does not have the mental equipment required to appreciate basic aspects of science, and that any attempt to communicate is an utter waste of time. They regard their colleagues' ventures into popularization as a mild form of corruption.

At the other extreme are the few scientists and science journalists who believe that they have solved the essential problem of popularizing science. Their monolithic complacency may annoy us, but it nevertheless deserves some sympathy. It is not always easy to live with the knowledge that one's writing and editing frequently leave much to be desired. On the other hand, insight and a fair share of humility may be helpful when it comes to setting higher standards and making a serious effort to meet them. I think it is reasonably evident by this time that higher standards are called for.

Thistle, chief public relations officer of the National Research Council of Canada, is neither frustrated nor complacent. Critical in a positive way, he indicates major limitations and then suggests that the situation is not as hopeless as it has been pictured. Indeed things could be a great deal worse, especially if we consider some of the things that are happening outside the laboratory.

Mr. Pfeiffer, a science writer, lives in New Hope, Pa.

One of the most awe-inspiring symptoms of the times is the prospect of publicity gone wild. We are being subjected to a mounting and almost incessant barrage of messages from experts in the fine art of pleading causes, lost and otherwise. A legion of organizations and institutions compete for our undivided attention. Like a crowd milling past the sideshows of a great amusement park, we are exposed to the shouts of many barkers.

Only a near-miracle—or a succession of sputniks and other overseas achievements—can account for the fact that the earnest and unexcited voice of the scientist, or the somewhat more compelling voice of the science writer, is heard increasingly in such a hubbub. To be sure, science may not be getting its full message across. But something important is beginning to come through, and the techniques for communicating a great deal more have long been in existence. I have no serious argument with Thistle's estimate that only about one hundredth of one percent of what scientists know finally reaches the layman, but, as he is quick to point out, that may not be a completely suitable measure of the problem.

Lopsided View

Most people who visit art galleries know very little about art by comparison with professional painters, yet they may appreciate what they see and be ready for deeper understanding. The problem is not how much the layman knows but the quality of his knowledge. If the right things are communicated, it is quite possible that one hundredth of one percent may be ample. The trouble, of course, is that we have not succeeded in conveying all the "right things." The image of the scientist in the eyes of recently surveyed high-school students, the popularity of pseudoscience and antiscience, the attitudes of many businessmen as indicated in off-the-cuff statements and in the advertisements they approve—these and other phenomena suggest that the public has an exquisitely lopsided view of science.

Science means "applied studies" to the vast majority of nonscientists, and to an appreciable proportion of scientists. The layman reads too little about basic research. What he does read may fail to get across, as I. I. Rabi of Columbia University indicated at a meeting of the American Institute of Physics: "There is hardly anybody in this room who has not had the frustrating experience of trying to explain what science is about to laymen, whether in government, in the universities, or to the ordinary educated layman, professional or businessman. Such is the spirit of the time that it is difficult, if not impossible, to communicate the feeling of dedication and reverence which all physicists have for our discipline."

Something is wrong here. We have unsuccessful communication where, on the face of it, one has every reason to expect success. In many ways it is far less difficult to convey feelings than facts. The layman has more experience with dedication and reverence than with most scientific concepts—and the odds are that the failure is ours rather than his. One reason for the failure, of course, is the scientists' "persistent refusal to learn the techniques of communication." The techniques are as familiar to professional writers and public-relations specialists like Thistle as standard laboratory methods are to professional investigators. Yet I know of no publication which consistently uses these techniques to present interesting and literate articles on basic research to the layman.

Concentration of Emphasis

Concentration of emphasis is perhaps the most important single technique, or principle, of effective writing. It is also the most widely and flagrantly neglected. Suppose a scientist wants to write an article on basic research, a common enough subject in times like these. Many ideas crowd into his mind. Basic research needs more adequate support. Basic research cannot flourish without an educational system capable of producing more scientists interested in the fundamental aspects of natural phenomena. Sooner or later, most basic research "pays off" in terms of practical results and technology. Basic research is a creative activity like music or painting. And so on.

These and a host of other ideas are well worth emphasizing. *But*, and the point is crucial, it is impossible to emphasize more than a single one of them and still communicate. An article must have a theme as well as a subject. This does not imply that the article must be devoted to one idea only (although such a practice frequently has much to recommend it.) Nevertheless, the job is to select as few ideas as possible—an absolute minimum—and then focus on one of them as the main theme of the article. This is what I mean by concentration of emphasis. It is essentially a problem of organizing, a problem which should be solved once and for all before the writing itself starts.

When a purportedly popular article is crammed with ideas, it tells us a great deal more about the author than it does about the subject. We see that the author writes for a very small audience consisting of himself, his colleages, and a few others whom he would like to impress. We see that in pursuing his private objectives, in satisfying his ego, he has forgotten the general reader. It is far better to discuss one point—and get it across—than to discuss many points and lose them all. The scientist often feels he must cover a subject comprehensively—and swamps the reader with a mass of unweighted information. Limiting one's scope calls for a certain amount of self-discipline, but that is the essence of communication.

Courtesy to the Reader

As far as the writing process itself is concerned, in the last analysis every device and every technique boils down to plain everyday courtesy to the reader. Taking special steps to attract and hold his interest is simply a form of good manners. We come to him with our problems or with information we want to share. We ask him, in effect, to drop his own concerns for a time and listen to ours. He is often ready to listen, perhaps more often than we realize.

With the best of intentions we may nevertheless bore him somewhat because, in a fundamental sense, we are all amateurs in popularizing science. But it is sheer rudeness not to try to make ourselves interesting and convey our enthusiasms. If we take the trouble to try, he will know it and will sympathize with our spirit and our message. If we do not try he will know that, too.

A warning is appropriate at this point. Techniques used at present in most classroom lectures do not necessarily apply to the writing of popular articles. In facing up to the "courtesy" problems, scientists are beginning to appreciate the fact that there are major differences between addressing a group of undergraduates and addressing general readers. Students represent a captive audience. Taking extra pains to interest them is not widely accepted as a vital and continuing part of educational practice, although the validity of this attitude could be argued. After all, the student must pay attention or suffer the consequences.

The uninterested layman passes quickly and happily to another article, or another magazine.

There are many ways of writing colorfully, warmly, and with dignity. One rule is never to let a generality stand by itself. It should be followed by examples and anecdotes chosen so as to make a broad point as clear and definite as possible. For instance, take the statement, "After considerable technical difficulties, a suitable experimental procedure was devised." Such statements cry for expansion. A serious attempt should be made to indicate in simple, straightforward terms the nature of the difficulties and the effort required to overcome them. The reader may be interested in this sort of information. It not only involves anecdotes, but also provides some concrete idea of what working in a laboratory means.

Another rule—Thistle calls it "the old rule of writing"—concerns the use of comparisons to help bring the reader closer to concepts which might otherwise seem remote and abstract. A comparison, whether brief or extended, is not something to brush off—to hurry past on the way to an important point. It should be a leisurely and stimulating excursion for readers. An article conceals little about the attitudes and feelings of its author. He can take his job seriously enough to put his imagination to work and present unusually appropriate analogies. His article will sparkle as a result.

But if he is not really interested in the job, his article will not really be interesting. Good comparisons can make all the difference between a first-rate and a mediocre style, between effective and futile communication. A trite metaphor or simile, for example, is unfortunate, for it is an infallible sign of laziness—and, again, lack of courtesy. Yet atoms are still "miniature solar systems" (even though the concept no longer holds in modern physics), amino acids are still "building blocks," spiral galaxies are still "fourth-of-July pinwheels." The list of well-worn analogies which were once original could be extended indefinitely. We need fresh analogies and the sort of thinking that produces them.

"Shorthand" Vocabulary

Finally, we turn to the much-discussed matter of vocabulary. It is difficult for the scientist, or the scientifically trained writer, to appreciate the full extent of the gap between himself and nonscientists when it comes to technical terminology. Certain words have become so familiar to him that he uses them without thinking, and consequently much of what passes for popular science is semitechnical writing at best. Thorough and most enlightening studies of this problem have recently been completed by W. E. Flood at the Institute of Education of the University of Birmingham in England. Space does not permit a detailed account of his methods and results, but a few examples may help indicate how far we have to go.

One study dealt with the British popular-science magazine *Discovery*, which is a notch or two less technical than the *Scientific American*. In an advertisement, *Discovery* makes the following statement: "Its articles are written by eminent scientists and technologists, but in language that nonspecialists can easily understand." The University of Birmingham study does not confirm this claim. It shows that articles in the magazine are liberally sprinkled with terms like *phosphatic, sedimentary rock, electrostatic, metamorphosed,* and *thermal ratio* —terms which, however evident their meaning to scientists, mean little or nothing to laymen.

Flood concludes: "This magazine . . . is certainly not wholly written in nontechnical language and a nonscientific reader would not understand it. It may well be that it is really intended for a different type of reader. . . . But the study shows the extent to which scientific terms are liable to creep into reading matter which is meant to be popular rather than academic." A similar conclusion holds for articles in the Penguin publications *Science News* and *New Biology*.

Attention may also be drawn to a related and equally serious problem. The following quotation from the British magazine *The Listener* illustrates a common kind of failure in communication: "I suppose every naturalist and biologist who has contemplated the extraordinary adaptive variety of the world of animals and plants, or has investigated the amazing perfection of a highly developed sense-organ, such as the eye or ear, must have been brought up short by a doubt —is it conceivable that a fortuitous concourse of variations can have been responsible for the adaptive perfection we see?" This sentence is so bad in so many ways that I wish it were less typical. It should have been written as at least three sentences. It includes a number of charming, antique, and totally uncalled-for words and turns of phrase. But these faults are relatively minor. The main point is that the sentence says something quite definite and significant to some scientists and, at the same time, says little to the scientifically unsophisticated reader. From the layman's point of view it is dense and condensed beyond intelligibility. Words like *adaptive* and *variations,* as they apply to evolutionary theory, are shorthand. They mean a great deal to biologists, somewhat less to physical and social scientists, and considerably less to the general public. Science is full of such words. In popular-science writing every one of them must be amplified and clarified, with the aid of examples and comparisons and all other available devices.

Fresh Efforts Needed

I have mentioned a few of the many well-tested and recognized techniques which, if more widely applied, would make popular science more popular. Thistle has discussed these and others. Still other problems require the development of entirely new techniques. To cite only one example, there is the problem of magnitudes—the distance of the Andromeda galaxy, the size of a virus, the brief existence of a meson, the time that has elapsed since the earth's formation or the coming of man. How can we convey a real feeling for the enormously large or the infinitesimally small? An article could be written on past efforts to deal with magnitudes, and on the increasing need for fresh efforts in the future.

It is encouraging to note that more scientists are approaching popular science seriously enough to consider using established methods of communication. But, to look ahead, established methods are not sufficient. We still do not know precisely how effective any single method is in practice, nor do we know enough about the comparative effectiveness of different methods. We do not know what problems most urgently require the use of new methods. In other words, popular science is itself an area that demands further research. As I have already indicated, we are all amateurs in this field.

The Library of Congress

Its science collection is one of the finest in the world, with broad reference services for readers.

John Sherrod

During the recent Parliament of Science organized by the American Association for the Advancement of Science, the group concerned with communication among scientists and communication of scientific ideas discussed, among other topics, that of libraries and library services. It was somewhat frustrating for me to listen to the participants discuss the science collections of a number of institutions, abstracting services, union catalogs, and other cooperative library projects and not to hear mentioned a truly great science collection which belongs, and is accessible, to all of the citizens of this country. In fact, I was dismayed to find in conversations with several of the participants that, almost unanimously, they were either totally unaware of or misinformed about one of the largest collections of science materials in the world and what we at the Library of Congress modestly hope is one of the finest of such collections.

It is my hope that this brief report on the science collections and services of the Library of Congress will be of interest or even possible benefit to readers of this journal. I cannot describe these national resources in a completely detached manner; neither can I assume credit for them, since they originated somewhat before my time. When, in 1815, the Congress purchased from Thomas Jefferson his great book collection, the Library acquired the finest private library in the New World and one assembled with a special eye to "natural philosophy." In the nearly one hundred and fifty years that have followed, the Library's interest in works of science and technology has never been lost.

Collections

The Library of Congress is a research library which exists primarily to serve the Congress and, by that token, to serve the whole Federal Government and thereafter, by means direct and indirect, the whole nation. These claims upon the Library and their relative priorities are considerations of vital importance to an understanding of its policies and practices as they relate to its collection and servicing of its science holdings.

The first great external stimulus to the collection of science literature by the Library came in 1866 when, by act of Congress, the Smithsonian Institution's "complete collection of the memoirs and transactions of learned societies throughout the world and an entire series of the most important scientific and literary periodicals" were transferred to the Library of Congress in order that they might be housed and serviced more adequately. The Library of Congress continued and greatly expanded the foreign exchange program begun by the Smithsonian. Today it has a worldwide network of more than 16,000 exchange agreements with foreign governments and private research centers, laboratories, universities, observatories, and other scientific and technical institutions, which brings in nearly half a million books, pamphlets, journals, and other materials annually.

The facilities of U.S. agencies, both here and abroad, are also utilized in adding to the collections. Through the operation of the copyright law, the Library receives all American publications and many foreign ones in the scientific field that are registered for copyright. Other materials are received through the operation of domestic exchange agreements, by transfer, by gift, and by purchase. Some 240 dealers outside this country have standing orders to acquire, for the Library, publications of importance, regardless of language or country of origin.

The size and scope of the Library's present scientific and technological holdings attest to the vigor with which acquisitions in these fields have been pursued. As of 30 June 1957, there were 1,447,000 volumes classified (in the Library sense) in science and technology, or about 25 percent of the Library's total classified collection. It is estimated that the number of journal titles in science and technology received currently in the Library of Congress is approximately 15,000. Its collections in the Russian and Eastern European languages are the most extensive in the world outside the Soviet Union and the Iron Curtain countries, and this superiority applies to science materials as well as to the literature in other fields. A check of the Library's science receipts against the Soviet national bibliography indicates that 60 percent of the Soviet materials currently published in the natural sciences and 41 percent of those published in technology are being obtained by the Library.

The Library's strength in these area-language categories does not argue any weakness in others. Diversified specialization has resulted in great strengths in many fields and facets of science. Preeminence in the literature of aeronautics and of the aeronautical sciences has been enjoyed by the Library for many years. It is also undoubtedly the best equipped library in the United States for geographic and cartographic research and possesses collections totaling 2,500,000 maps and 20,000 atlases. It is well endowed with works of bibliography and has concentrated on collecting and maintaining book and card indexes, abstracting journals, and other finding and analytical aids relating to the literature of science and technology.

As the periodical article was the chosen vehicle for disseminating information in the 19th century, so the technical report has become the medium of today, and the Library has in recent years accumulated many tens of thousands of technical reports from a wide variety of sources and in nearly as great a variety of forms. These reports emanate chiefly from Government or Government-sponsored research, with a high proportion related to military programs. Wherever practicable, reports are acquired in their original printed or near-print form, but many are received on microfilm and microcards and are serviced through the use of viewers or through photoenlargement. Some of the principal segments of this large aggregation of reports include the publications issued by the Atomic Energy Commission and the National Advisory Committee for Aeronautics and the publications distributed by such organizations as the Office of Technical Services of the Department of Commerce.

The author is chief of the Science and Technology Division of the Library of Congress, Washington, D.C.

Through analysis, description, and the dissemination of information about its holdings in science and technology, as well as in other fields, the Library makes its national collections meaningful and available to the Government and the nation. Its system of subject classification, which is constantly being revised and kept up-to-date, is used by the major research libraries of the country. The Library prints and distributes catalog cards describing not only its own holdings but those of many other libraries. The essential nature of this service is evidenced by the fact that there are nearly 10,000 subscribers to it. The Library also maintains the National Union Catalog, containing cards locating research materials in 700 North American libraries, and it publishes the *National Union Catalog* and other catalogs in book form. To make information about acquisitions in certain fields, including science and technology, quickly available, the Library publishes the following monthly accessions lists: *New Serial Titles,* listing the periodicals received by the Library of Congress and some 200 other cooperating libraries, the *Monthly Index of Russian Accessions* (MIRA), the *East European Accessions Index* (EEAI), and the *Southern Asia Accessions List.* Out of a one-year accumulation of 11,709 monographs included in the *Monthly Index of Russian Accessions,* 6006 were classed as science and technology; of 8918 periodical issues listed (not including newspapers), 4800 were in the same fields. In one year (1956), the *East European Accessions List* described 1522 monographs and 9946 periodical issues in science and technology.

Science and Technology Division

These bibliographic endeavors make a formidable contribution to the nation's scientific effort, but they do not begin to exhaust the Library's investment in this area. The Library has also facilitated the widest possible exploitation of its resources, within the limits of budget, space, and its primary commitment of service to Congress. In addition to its own special divisions to provide reference and bibliographic services, the Library has since World War II administered several large-scale projects on funds transferred from the Department of Defense to provide analytical, abstracting, and bibliographic services on scientific and technical literature, including report literature, as well as other materials in the Library that need to be intensively utilized in undertakings important to the security of the United States and the Free World.

The focal point of subject competence within the Library in all fields of science and technology is the Science and Technology Division, which has primary responsibility for reference and bibliographical services and for advising on the acquisition of all pertinent scientific and technological materials. Established in 1949 with a small staff to plan and develop a science program, the division has assumed an increasingly active role in meeting the Library's growing obligations to provide its users with scientific information.

The primary function of the division is to provide general and advanced science reference service. The recent upsurge in interest in science is reflected in the growth of the reference service provided by the division: this growth has been approximately 400 percent in the last year.

Science reference service to readers is provided in the Science Reading Room, which is operated on a 7-day-a-week basis by staff members who are particularly familiar with the collections. Inquiries of a general nature received by telephone or mail are also handled here. Inquiries of a more advanced nature are handled by staff members who have had specialized training in the various fields of science.

An open catalog to the extensive report collection referred to above is maintained in the division. Supplemental support is provided by the National Science Foundation to expand the report collections and the personal reference services offered with respect to them. The Foundation also supports an exploratory program in the Oriental science literature, particularly Japanese literature. It can be hoped that such a study will result in the publication of critical bibliographies of this literature. An expanded program of a similar nature is also contemplated in Slavic science literature.

The primary responsibility of the division in the field of bibliography is to assure the effective use of the scientific and technical literature in the Library. Current activities include the preparation of a bibliography on the International Geophysical Year, a world list of aeronautical serial publications, and a list of scientific and technical serials currently received by the Library. Several continuing bibliographies are also being prepared for agencies within the Department of Defense. An example of such a bibliography is one being prepared for the Snow Ice and Permafrost Research Establishment of the U.S. Army Corps of Engineers. This bibliography, in progress since 1951, has brought under bibliographical control essentially all of the world's technical literature on the subjects of snow, ice, frozen ground, and related military and engineering topics. To date, more than 16,000 abstracts have been published in 12 volumes. These abstracts cover material published since the 18th century in 24 different languages.

The basic purpose of the Division's acquisitions activities is to maintain and further develop the collections of the Library in science and technology. The major effort in this area is expended in keeping current in serial publications and selecting for the collections from the 200 or so that appear each month for the first time. Although recommendations are made on a selective basis, pointed effort is made to develop a sample file of those serials of minimal importance which, for one reason or another, are not recommended for continued acquisition.

The division, as an information center, is constantly studying the application of improved and advanced documentation techniques in order better to cope with the increasing volume of published literature. The division desires to maintain active contact with research groups and other organizations in order to be informed concerning their interests and activities and the substantive output of research as it affects the literature. The division, in turn, publicizes its own activities, programs, and services, and, as far as possible, makes these available to the public.

The Library of Congress serves in many ways as the national library of the United States. This brief examination of its resources, its services, and its plans in the field of science and technology should serve to demonstrate that, by the broadness of its base, the preeminence of its collections in many disciplines, its long record as an authority in bibliographic know-how, and the rich competence and variety of its staff, the Library of Congress is also the national library of science.

It can be seen that the Library of Congress has a vast investment in the literature of science. It must follow that science, which in great measure relies for its own stimulation and growth on that literature, has an equally large investment in the Library of Congress.

News of Science

Economic Status of Professors

A program for the improvement of the economic status of college professors and for the maintenance of high standards is outlined in the current [Spring 1958] issue of the *AAUP Bulletin*, official publication of the American Association of University Professors. The association proposes that the "absolute highest priority in the use of available funds be given to raising faculty salaries, with the goal of doubling the average level within 5 to 10 years." This recommendation is identical with the one made to "every board of trustees, every legislature, and all others responsible for academic budgets" by the President's Committee on Education Beyond the High School in its "Second Report to the President," July 1957.

The association also proposes that administrators and teachers rate their institutions' scales of minimum salaries by reference to a table containing minimum salaries in six grades, A through F. A grade of A will be given if the following minimum salaries have been attained: professors $12,000, associate professors $8750, assistant professors $6750, and instructors $5000. Minimum salaries by ranks for grade B are $10,000, $7750, $6000, and $4500. For grade C they are $8750, $6750, $5250, and $4000; for grade D, $7500, $6000, $4750, and $3750; for grade E, $6250, $5250, $4250, and $3500; and for grade F, $5250, $4500, $3750, and $3250.

The association makes this proposal in the belief that a graded salary scale will encourage the various sections of the public to increase their support of efforts to raise faculty salaries at institutions in which they are interested. A special grade of AA is included in the table as a goal for institutions which find their present salary schedules in grades A or B. Minimum salaries in this grade are $14,000, $10,000, $7750, and $6000 for the four academic ranks.

The table of minimum salaries, which the association proposes as a guide in this self-rating system, was determined after examination of the data in two reliable studies of academic salaries recently completed. One is the latest biennial salary report of the AAUP, which is based upon the 1957–58 salaries reported by 39 selected institutions having higher than average academic salaries in recent years.

The other study, entitled "Higher Education Planning and Management Data, 1957–58," is based upon replies from 1146 colleges and universities. It was compiled under the direction of W. Robert Bokelman of the U.S. Office of Education and published by the U.S. Government Printing Office.

These studies, together with press releases on recent salary rises, show that current (1957–58) minimum scales of American institutions are found in all grades except AA, which is a 1960 target for present grade A and B scales. Some institutions may have low grades for their minimum salaries but would qualify for much better evaluations on other scores. Anticipating this disappointment, the association recommends the publication of supplementary information on average salaries, student-faculty ratios, and instructional expenses per student.

Colleges and universities are urged to adopt the following practices in the determination of faculty salaries: (i) the preparation and announcement of a salary schedule, (ii) provision for a meaningful annual audit and fiscal report on operations, and (iii) provision for faculty consultation on the budget, either by faculty representation on institutional budget committees or through a faculty budget committee created to advise with the appropriate officers and committees.

The association also proposes that long-range planning committees composed of faculty, or having faculty representation, be established in institutions now without them. These committees should deal with the critical problems associated with quality maintenance and with expansion. They would study such problems as the limit on the number of students to be enrolled, the staff and space requirements for the planned number of students, the budgetary requirements of planned salary increases and expansion of staff and facilities, the priorities to be established among competing needs, the extent to which the institution should continue to provide education at less than cost, and the means of aiding the superior but needy student.

A fifth proposal by the association is that state or regional conferences of professors be organized in areas where conditions justify them. Such conferences would have as their objectives the encouragement of long-range planning in all area institutions, the pooling of information concerning institutional plans, and the conserving of the educational dollar by encouraging and supporting the full use of existing institutions. These state or regional conferences could also serve a liaison function between faculties and state commissions or other planning groups in the area. They would be available to collect data on higher education in the region, as needed.

The chairman of the AAUP Committee on the Economic Status of the Profession is William A. Neiswanger, professor of economics at the University of Illinois.

Science Writing at Columbia

Columbia University's Advanced Science Writing Program will start next September on a scale enlarged over that originally announced [*Science* **127**, 228 (31 Jan. 1958)]. This is possible because of a 3-year $100,000 grant from the Rockefeller Foundation. The pilot financing was a 2-year, $70,000 grant from the Alfred P. Sloan Foundation. The combined financial support provides for six to eight fellowships the first year and ten or more in each of the two following years. Within a month of the December announcement of the program, nearly 150 inquiries and applications were received by the Graduate School of Journalism.

The stipends have been set high; they will cover, in addition to university tuition and fees, up to approximately $4400 travel and living expenses for the academic year. It is possible that there will be one or more of what might be called "corporate" fellowships, under which a publication or industrial organization will pay the tuition and fees and make its own salary arrangements with a qualified employee during his semester or year in the program.

The program will be kept as flexible as possible to meet the individual needs of the participants. In one case, for example, a writer might want to specialize in one phase of science, and thus would concentrate his studies in that area. Another, planning a career of "general assignment" science reporting, would take courses in several areas, either to get advanced training or to acquire knowledge in fields not studied in undergraduate days.

In general the program will include participation in regular or specially arranged science courses or seminars under Columbia's faculties of pure science,

medicine or engineering; organized study of selected research and development projects in the New York area; special meetings with both university and industrial research leaders; and special writing, some of it for publication. All participants will be expected to undertake a major research writing project in addition to regularly assigned writing.

At least for the first year, there will be no set academic, experience, or age requirements for admission, but it appears likely that the initial participants will be college graduates with good academic records and fairly broad writing experience. Each case, however, is being studied on the basis of the applicant's potential as well as past performance. It is expected the selections will be made by 1 May. Because the Advanced Program is new, detailed plans will not be completed until the participants have been selected.

It is possible that, as the program develops, the school will offer writing courses for selected graduate students in the faculties of pure science, medicine, and engineering, courses aimed at making the scientists better able to communicate with each other as well as the public. It is also possible that special short-term conferences for experienced science writers from all parts of the country can later be arranged, perhaps using facilities such as the university's Arden House, home of the American Assembly. Here the writers would live and work together—probably for 1-week periods—in a concentrated program designed to give thorough briefings on the newest scientific developments and the techniques of presenting them. Such conferences would bring together the best minds in research and development and those actively engaged in reporting.

For guidance on policy matters, the program will have an advisory board with members from the Columbia faculties, publishing, industry, research organizations, and the National Association of Science Writers. The program has been greeted with good will and enthusiasm by scientists from all over the country. Their offers of cooperation auger well for the experiment being made possible by the Sloan and Rockefeller foundations.

JOHN FOSTER
Graduate School of Journalism, Columbia University

Army College Program

The Army has announced that it will offer to finance 4 years of college education for soldiers who agree to stay in the service for 12 years. The program for enlisted personnel—soldiers or WAC—will be similar to that carried out in the education of officers in past years. It will encourage career service in the Army, while at the same time making select soldiers better educated. Participation will be strictly on a voluntary basis, and a soldier who wants to get into the program will not have to sign up for the whole 12-year enlistment right at the start.

Soldiers in college will remain on active duty with their GI pay and allowances and the government will pay the education costs. Applications for the first year will be made this spring, and about 300 enlisted personnel are expected to take part.

Cattell Papers

The papers of Joseph McKeen Cattell have been presented to the Library of Congress by his son, Jaques Cattell of Lancaster, Pa. Numbering some 15,000 items, they range over his career from his years as a student at Leipzig, Germany, in the early 1880's to his retirement in the 1940's.

Correspondence concerning Cattell's editorship of the journals *Science*, *The Psychological Review* (which he founded), *The Scientific Monthly*, *Popular Science Monthly*, and *School and Society*, and the biographical dictionaries *American Men of Science* and *Leaders in Education*, makes up the bulk of the collection.

In addition, the papers include material on Cattell's research, writing, and teaching in the field of experimental psychology at Columbia University in the 1890's, his operation of the Science Press, and the activities of the American Association for the Advancement of Science, of which he was president in 1924. When they have been organized, the papers will be available for use in the manuscript division of the Library of Congress.

Index to Plant Chromosome Numbers

The need for up-to-date coverage of the literature dealing with plant chromosome numbers has led to an undertaking designed to compile and publish in annual installments a chromosome index for the entire plant kingdom. This is being done by a group of botanists who are reviewing some 200 journals and are listing all original chromosome counts occurring therein, except those resulting from endopolyploidy or deviating because of experimental treatment.

The index, compiled from the journals of a single year, will be published annually within the shortest possible time after the last issue of each journal is available. In addition, a supplement is planned that will contain counts published in previous years but hitherto not indexed. Each issue of the index will contain a complete bibliography for the counts included in that number.

The first issue, in part-supported by a grant from the University Research Council of the University of North Carolina, will be ready for distribution in May 1958. It will cover the 1956 journals, from which more than 2000 listings have been taken. It is being produced by offset process on 8½-by-11-inch paper, punched for looseleaf binders. The price is $1. Advance orders would be helpful in determining the number of copies to print. Orders *must* be accompanied by payment and may be sent to C. Ritchie Bell, Department of Botany, University of North Carolina, Chapel Hill, N.C. Further information can be obtained from Marion S. Cave, Department of Botany, University of California, Berkeley 4, Calif.

Chemical Crystallography

Pennsylvania State University has announced establishment of the Groth Institute to serve as world center for revision of *Chemical Crystallography*, encyclopedia of crystal chemistry and physics. The institute will operate within the College of Chemistry and Physics under the direction of Ray Pepinsky, research professor of physics and director of the X-Ray and Crystal Structures Laboratory. John A. Sauer, head of the department of physics, will serve as the university's administrative representative.

Chemical Crystallography was issued by the great German crystal chemist, physicist, and mineralogist, Paul Heinrich Ritter von Groth, in the years between 1906 and 1919. Pointing out that Groth's compilation of crystal properties still is of tremendous value to natural scientists, Pepinsky notes that it was conceived before the discovery of x-ray diffraction and crystal structure analysis and therefore contains little information compared to that now available relating crystal structures and chemical and physical properties.

Pepinsky will serve as editor-in-chief of the encyclopedia. The editorial board will consist of chemists, physicists, mineralogists, and metallographers from all parts of the world. An advisory board will be chosen, selected from editors and special compilations of chemical, physical and mineralogical data. A board of trustees, composed of representatives from supporting agencies, also will be selected.

The first revision of the encyclopedia is expected to take at least 10 years. The first 2 years will be concerned almost entirely with data collation. Since infor-

mation on crystals is being collected so rapidly, it is anticipated that further revisions will be required continually.

A conference of American editors, contributors and other interested crystallographers will be held at the university 25–26 April under the auspices of the Solid State Sciences Division of the Air Force Office of Scientific Research. Plans for the institute, which have been under consideration for several years, were furthered by a conference of leading crystallographers at Harvard University in 1956; two trips to Europe by Dr. Pepinsky in 1957; and discussions at the Congress of the International Union of Crystallography in Montreal last June. Pepinsky will go to Europe in June under a Guggenheim fellowship and a Smith-Mundt grant from the U.S. State Department. There he will complete the organization of the institute's editorial board. He also will make arrangements for contributions from many laboratories and will participate in conferences concerning the newly developed punch card data-handling methods.

Orthoptera

Current Research on Orthoptera is a new publication of the Anti-Locust Research Centre, 1 Princes Gate, London S.W. 7. It is a list of workers now engaged in research on Orthoptera, giving their names and addresses, and indicating the nature of their current work. Information was computed from a questionnaire that was sent out in 1956 to institutions and persons all over the world known to be engaged in research in the field. Out of 250 questionnaires sent, 215 were returned by workers in 37 different countries. It is hoped to issue, from time to time, similar lists of new research workers.

Grants, Fellowships, and Awards

Allergy. The Scientific and Educational Council of the Allergy Foundation of America has announced the availability of quarterly or summer scholarships at $500 each in approved medical schools in the United States and Canada. These scholarships are available to students who have creditably completed their second or third year in medical school, and are to be for a minimum of 8 weeks of training in clinical and research allergy. Each medical school has been invited to submit the name of one applicant through the dean's office. Application must be accompanied by a supporting letter from the dean that outlines the candidate's medical school record, the proposed research project, and under whose sponsorship it will be carried out. Applications from individuals will not be considered. All applications must be sent before 30 April to Dr. Robert A. Cooke, Chairman, Scientific & Educational Council, Allergy Foundation of America, 801 Second Ave., New York 17, N.Y.

Life Sciences. The Division of Biological and Medical Sciences of the National Science Foundation has announced that the next closing date for receipt of basic research proposals in the life sciences is 15 May. Proposals received prior to that date will be reviewed at the summer meetings of the foundation's advisory panels and disposition will be made approximately 4 months following the closing date. Proposals received after the 15 May closing date will be reviewed following the fall closing date of 15 September. Inquiries should be addressed to National Science Foundation, Washington 25, D.C.

Nuclear Science. The Atomic Energy Commission is inviting American colleges and universities to submit proposals by 1 May for the fifth series of grants to provide laboratory equipment for use in nuclear science and engineering instruction. Copies of the criteria used in evaluating proposals may be obtained from the Director, Division of Reactor Development, Atomic Energy Commission, Washington, D.C. Although the commission invites proposals at any time, cut-off dates are announced periodically.

Physical Sciences. The National Science Foundation has annonunced that physical scientists, mathematicians, and engineers who wish support to attend international meetings concerned with subjects in the physical sciences should submit applications for financial assistance prior to 30 June for travel between 1 October and 31 March, and prior to 31 December for travel between 1 April and 30 September. Requests for application forms should be addressed to the Assistant Director for Mathematical, Physical, and Engineering Sciences, National Science Foundation, Washington 25, D.C. Separate announcements will be made in appropriate journals with reference to specific international meetings dealing with biological and medical subjects for which the foundation can extend travel aid and the dates when travel applications will be received.

Steroid biochemistry. Applications are now being accepted for the third course in the training program for steroid biochemistry which will begin on 1 October. The program is sponsored by the National Cancer Institute of the National Institutes of Health to provide specialized training for individuals interested in steroid investigations. The program is conducted by personnel at the Worcester Foundation for Experimental Biology with the department of chemistry, Clark University, Worcester, Mass., and the department of biochemistry, College of Medicine, University of Utah, Salt Lake City.

Two groups of candidates will be selected for training. Postdoctoral candidates having an M.D. or a Ph.D. degree will receive $5000 for a 1-year training period extending from 1 October 1958 through 31 September 1959. The course will consist of laboratory instruction and lectures covering the theoretical and practical aspects of steroid research, and a period of research on a steroid problem with an established investigator.

Candidates having a B.S. or M.S. degree or equivalent training will receive $1500 for a 6-month period extending from 1 October 1958 through 31 March 1959. The predoctoral program is intended to provide competency in the analysis of steroid substances for research and clinical laboratories.

The closing date for applications will be 1 June 1958. Requests for applications should be made to the Training Program for Steroid Biochemistry, Department of Chemistry, Clark University, Worcester, Mass., or to Dr. Kristen Eik-Nes, Department of Biochemistry, College of Medicine, University of Utah, Salt Lake City, Utah.

Scientists in the News

GEORGE W. CORNER of the Rockefeller Institute for Medical Research has been selected as the recipient of the $5000 Passano Foundation Award for 1958. On 25 June, during the convention of the American Medical Association in San Francisco, a reception and dinner will be held for Corner at the St. Francis Hotel. He is being honored "for his long and continuing researches and their many fruitful contributions to the better understanding of mammalian anatomy and physiology, with particular emphasis on human reproduction." From 1940 until his retirement in 1955, Corner was director of the department of embryology at the Carnegie Institution. At present he is engaged in writing a history of the Rockefeller Institute for Medical Research.

F. P. BOWDEN of the Laboratory of Physical Chemistry, Cambridge, England, will arrive in the United States about 10 May, for a visit to Washington, D.C., and other cities.

The Department of Defense Distinguished Civilian Service Award, highest honor conferred on civilian employes by the department, has been presented to the following men: PAUL A. SIPLE, scientific adviser to the chief of research and development, deputy chief of staff, research and development, Department of the Army, "for his significant contri-

bution throughout the past twenty years to scientific knowledge of the Antarctic region"; GREGORY K. HARTMANN, technical director, U.S. Naval Ordnance Laboratory, "in recognition of his technical and administrative direction of the Naval Ordnance Laboratory and his contributions to military science and technology, particularly in the fields of aeroballistics, physics, and explosives research"; CLAY G. HUFF, parasitologist, U.S. Naval Medical Research Institute, National Naval Medical Center, "in recognition of exceptional work in parasitology and particularly in original research on the malarial parasite"; JOHN W. HEIM, chief, physiology branch, Aero Medical Laboratory, Air Research and Development Command, "for 20 years of pioneering in the field of aeromedical research which provided basic knowledge required for high altitude manned flight and manned space flight and for his initiative in developing the *Handbook of Biological Data*"; DOYLE NORTHRUP, Headquarters Command, Department of the Air Force, "in recognition of pre-eminent service to the Department of Defense from February 1948 to November 1957 during which he served as technical director of an Air Force atomic energy organization directing and coordinating efforts of multiple military and civilian agencies in a scientific program of great national significance"; and CHARLES S. WEAVER, director, Office of Foreign Programs, Office of the Assistant Secretary of Defense (Research and Engineering), "for outstanding efforts in improving technical cooperation between the United States and its allies in the areas of science, intelligence, and security."

GEORGES H. WERNER, formerly research assistant professor of microbiology in medicine at the University of Pennsylvania, has been put in charge of the Virus Research Division of the Rhone–Poulenc Chemical Corporation, Paris, France, effective 1 May.

W. W. GRIGORIEFF, chairman of the University Relations Division of the Oak Ridge Institute of Nuclear Studies, will take a 1-year leave of absence to become chief of the exchange branch of the International Atomic Energy Agency's Division of Exchange and Training of Scientists and Experts. He will leave Oak Ridge on 15 May and will assume his new duties 1 June at IAEA headquarters in Vienna.

STEPHEN ROTHMAN will deliver the first Dohi Memorial Lecture in Niigata, Japan, on 24 May at the 57th annual meeting of the Japanese Dermatological Association. The lectureship, which was established at the suggestion of Thomas B. Fitzpatrick of Portland and K. Kitamura and T. Kobori of Tokyo, honors Professor Keizo Dohi (1866–1931), the founder of Japanese dermatology. Each year a distinguished dermatologist is to be invited to the annual meeting of the Japanese Dermatological Association or to some other dermatological meeting to be held in Japan. The Dohi Memorial Lectureship has been created to provide for an exchange of lectures and discussion between Japanese and foreign dermatologists to promote progress in Japanese and world dermatology.

THEODORE H. REED has been appointed director of the Smithsonian Institution's National Zoological Park, Washington, D.C. Reed was first appointed to the staff in July 1955 as chief veterinarian, a position he held until 1 November 1956, when he became acting director upon the retirement of William Mann.

CLIFFORD C. CARR, professor of electrical engineering and chairman of the department of electrical engineering at the Pratt Institute Engineering School, has been named assistant dean of the Engineering School.

Four scientists from the United States were invited to speak before the Swiss Academy of Sciences, Lausanne, Switzerland, 27–29 March: WILLARD F. LIBBY, commissioner, U.S. Atomic Energy Commission; J. LAWRENCE KULP, Lamont Geological Observatory, Columbia University; ALEXANDER HOLLAENDER, Biology Division, Oak Ridge National Laboratory; and WRIGHT H. LANGHAM, assistant health division leader for biomedical research at the University of California's Los Alamos Scientific Laboratory.

Recent Deaths

CARLETON R. BALL, Washington, D.C.; 84; formerly agronomist with the U.S. Department of Agriculture, recently a member of the coordinating committee of the Tennessee Valley Authority, Department of Agriculture and State Experiment Stations; specialist in genus *Salix*; 2 Feb.

RAFE C. CHAFFIN, Los Angeles, Calif.; 76; surgeon and professor emeritus in gynecology at the College of Medical Evangelists; founder of the American College of Surgeons, vice regent and fellow of the International College of Surgeons; invented the Chaffin drainage tube, which became a standard tool in abdominal surgery; 23 Mar.

W. IRVING CLARK, Worcester, Mass.; 78; medical director at the Norton Company, Worcester; instructor in industrial medicine at Harvard University from 1919 to 1931 and clinical professor in 1940; 5 Apr.

LEONARD W. GOSS, Columbus, Ohio; 80; retired professor of veterinary pathology at Ohio State University, where he taught for 22 years; 2 Apr.

A. SPENCER KAUFMAN, Philadelphia, Pa.; 75; clinical professor of otology at Jefferson Medical College and chief of the ear, nose, and throat department of the Einstein Center, Northern Division; 30 Mar.

HARVEY L. LANTZ, Ames, Iowa; 70; horticulturist at Iowa State College since 1917; professor of horticulture and head of the pomology department of the Agricultural Experiment Station since 1945; 3 Apr.

KARL LARK-HOROVITZ, Lafayette, Ind.; 65; head of the department of physics at Purdue University since 1932 and a member of the AAAS Editorial Board since 1949; general secretary of the AAAS from 1947 to 1949; original member of the AAAS Cooperative Committee on the Teaching of Science and Mathematics and chairman of the committee from 1945 to 1950; 14 Apr.

D. T. MACDOUGAL, Carmel, Calif.; 93; director, department of botanical research, Carnegie Institution, 1905–28, and research associate, 1928–34; AAAS general secretary, 1922–24, and president of the AAAS Southwest Division, 1922; 22 Feb.

OTTO SANDVIK, Rochester, N.Y.; 65; head of the physics division at Kodak Research Laboratories in Rochester; known for his work on the physics of the developed image, spectral sensitivity of photographic emulsions, and the photographic aspects of sound recording; 8 Oct. 1957.

RICHARD A. STUDHALTER, Lubbock, Tex.; 70; professor of botany and first head of department of biology, Texas Technological College; specialist in *Riella* and *Ephedra*, and in tree rings; 28 Mar.

JAMES SWINBURNE, London, England; 100; a pioneer in plastic development; his work on phenolic resins at the beginning of the century was a major contribution to the development of the modern plastic industry; founder of Bakelite, Ltd.; 30 Mar.

HORATIO C. WOOD, JR., Philadelphia, Pa.; 84; professor emeritus of pharmacology and therapeutics at the University of Pennsylvania Hospital and professor emeritus of pharmacology at the Philadelphia College of Pharmacy and Science; 31 Mar.

Erratum: In the article "Color Defect and Color Theory" by C. H. Graham and Y. Hsia [*Science* 127, 675 (28 March 1958)], the last 13 lines in column 3, page 679, appear in reverse order. The bottom line should be read first, the second line from the bottom should be read second, and finally, the 13th line from the bottom should be read last.

Book Reviews

Colleges for Our Land and Time. The land-grant idea in American education. Edward Danforth Eddy, Jr. Harpers, New York, 1957. 328 pp. $4.50.

Three dates constitute milestones in the history of the land-grant institutions of our nation. The first was 1862, when, in the midst of the Civil War, President Lincoln signed the bill, sponsored by Representative J. M. Morrill of Vermont, "to donate public lands to the several states and territories which may provide colleges for the benefit of agriculture and the mechanic arts." In 1887, the second major development came with the passage by the Congress of the Hatch Act (named for a representative from Missouri), providing $15,000 of federal support annually to each state and territory that would create an "agricultural experiment station, in order to aid in acquiring and diffusing among the peoples of the United States, useful and practical information on subjects connected with agriculture, and to promote scientific investigation and experiment respecting the principles and applications of agricultural sciences."

The third milestone came in 1914, when federal grants were given to states and territories having land-grant colleges, to aid in "cooperative agricultural extension work [which] shall consist of the giving of instruction and practical demonstrations in agriculture and home economics to persons not attending or resident in said colleges in the several communities, and imparting to such persons information on said subjects through field demonstrations, publications and otherwise."

E. D. Eddy has summarized the facts regarding the inception of the land-grant institutions, their development over the 100 years since 1862, and their current status. It is a record of remarkable growth in both size and influence. For years to come this book is certain to be a source that will be frequently referred to by those concerned with the historical record.

Previous to 1860, nearly all of the colleges in this country were church-sponsored and denominationally inspired. Their graduates became clergymen, schoolmasters, doctors, and lawyers. Science received little emphasis and usually was treated like philosophy, theology, the dead languages, and mathematics—something to be studied and learned by rote, which remained essentially the same decade after decade.

Attendance at college gave respectability and social prestige but very little, if any, specific training that would enable the graduate to be more efficient in the vocations upon which the economic progress of society depended. Harvard College, as late as 1850, was declared by Louis Agassiz to be "a respectable high school where they taught the dregs of learning."

Although the new land-grant colleges were characterized by much more zeal than experience or skill in the devising of ways to attain their goals, they were dissatisfied with the colleges then in existence, hopeful that science might be used more effectively to advance the public welfare (particularly in regard to agriculture), and determined that higher education should no longer be a monopoly of the socially elite.

But it is not surprising that very few students sought entry to the new land-grant colleges and that farmers had little confidence in what professors could do for agriculture. Many of the early programs of instruction in agriculture were both ridiculous and pathetic.

The professors, trained in the classical institutions, knew little science, and what they taught was not focused on the physical and biological problems confronting farm operators. The first professor of agriculture at the University of California, according to E. J. Wickson, gave "a thorough course in fruit growing in the Garden of Eden, passing spiritedly to grain growing in Egypt and the conditions surrounding the corner in sorghum which Joseph contrived for Rameses II, pausing to look carefully into the dairy practices of the Scythians, and was rapidly approaching the relatively modern cabbage growing of Cincinnatus when, as tradition declares, both instructor and pupils fell asleep while pursuing dry-farming by the encyclopedestrian method of teaching."

The clear need was for new scientific facts and the development of improved farm practices based on [illegible]eful research. With the creation of the [illegible]gricultural experiment stations the pr[illegible]essors of agriculture learned new fact[illegible]nd new processes that would benefit [illegible]armers. Their research findings also e[illegible]bled the professors to put new subst[illegible]ace into their college teaching, and th[illegible]eby they won the interest and respec[illegible] of increasing numbers of students.

As research showed the [illegible]way, the agricultural colleges became more specialized. No longer were there just professors of agriculture, but whole new departments were created in agricultural chemistry, dairy husbandry, animal husbandry, agronomy, horticulture, entomology, and a dozen other fields of science related to plant and animal production.

By 1955 the 69 land-grant institutions (including 17 for Negro students) enrolled more than 20 percent of the students in the nation's degree-granting colleges and universities. They awarded nearly 40 percent of all doctoral degrees granted in that year. Over half of all officers commissioned in 1955 through the Reserve Officers' Training Corps were from the land-grant institutions.

But for those who seek a critical analysis of the hows and whys of the accomplishments and shortcomings of the land-grant institutions, this publication will not supply the answers. For example, it does not tell in what ways, if any, the progress of the larger state universities incorporated in the land-grant system has been better than or different from that of other large state universities. Whether the "mechanic arts" part of a land-grant institution is significantly different from the engineering school in other universities, the book does not say. Neither does it explain why, with only a few exceptions, the land-grant institutions in the older and more densely populated states did not experience anything like the relative growth and prestige that their sister institutions in other parts of the nation did. It would be helpful, also, to know whether academic freedom and the support of fundamental research have, on the average, fared as well in these institutions of higher education supported by the public as in those depending upon religious groups or other private sources.

Many of us would welcome a critical review of the record of our land-grant colleges and universities, which the world recognizes as being, in so many ways, unique and also so peculiarly American. We believe such an analysis would disclose much that would be enlightening, and that it would likewise confirm the confidence so many persons have in these land-grant institutions.

NOBLE CLARK

College of Agriculture, University of Wisconsin

The Sh… pearean Ciphers Examined. An a… s of cryptographic systems used … vidence that some author other … William Shakespeare wrote the pl… ommonly attributed to him. Willi… F. Friedman and Elizabeth S. Fr… man. Cambridge University Press, … York, 1957. xvii + 303 pp. Illus. $5.

The Friedmans have attempted a scientific examination of an irrational cult. That is at once the strength and the weakness of their book.

An astonishing number of people, most of them educated and many of them distinguished scholars, have denied that Shakespeare wrote the plays attributed to him by his fellow actors Heminge and Condell and by many others of his contemporaries. Yet a few of the witnesses, like the first important one, Robert Greene, were so hostile that they would have missed no opportunity to reveal a hoax had they suspected its existence. Why has their testimony been disputed? The idolatry of Shakespeare's extreme admirers during the 19th century inevitably produced sceptics who pointed out that the idol was only a simple country boy from Stratford who could not possibly have possessed the supernatural wisdom claimed for the author of the plays. But the sceptics themselves needed a faith. Looking for a more suitable candidate for apotheosis, they lighted upon Francis Bacon, whom they supposed the founder of modern science and the wisest man of his age. The Baconian hypothesis is, in its way, a minor heresy thrown off by the modern worship of science. But other claimants have also been advanced—the Earl of Oxford, Queen Elizabeth, and most recently, Christopher Marlowe.

This denial of Shakespeare's authorship is fundamentally a faith held in defiance of evidence that scholars trained in historical research find thoroughly convincing. But the Baconians, too, sought for evidence to support their faith; and, since Bacon had himself described an extraordinarily good cipher based upon the use of two fonts of type in the same text, they have looked for ciphers, as well as acrostics and anagrams, in the works of Shakespeare and Bacon. Different type faces undoubtedly appear there, as they do in almost all Elizabethan books. Proponents of the ciphers have claimed to find not only revelations that Bacon (or others) wrote the plays but also various historical revelations, often scandalous, and even other plays concealed as cryptograms in the folio text of Shakespeare. These claims the Friedmans study in the light of their expert knowledge and experience (they are among the most distinguished living cryptologists), and they easily demonstrate that the so-called ciphers meet none of the tests imposed by a good cryptologist and are so loosely conceived that they could produce any result subconsciously desired by their proponents. As a scientific examination of evidence, this book is devastating, and it would settle the controversy forever if a faith could be destroyed by rational arguments.

But the faith itself is, to the student of human behavior, more interesting than the spurious ciphers developed to buttress it. The Friedmans furnish only glimpses into the minds of the decoders. All of them invite curiosity as to their mental processes; one, at least, betrays, in the revelations that she detected, symptoms of sexual pathology. A fuller discussion of the human phenomena behind the alleged cryptograms would have made the book, for one reader at least, more interesting and more significant as a contribution to learning.

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The World Beneath the Waves. Gilbert Doukan. Translated by A. and R. M. Case. De Graff, New York, 1957. 356 pp. Illus. $6.

This volume is a comprehensive compendium in which are described the efforts of man to penetrate beneath the waters, both of the shore and of the open ocean, and there to carry on various activities. The major emphasis is on skin diving and hunting, and the locale is chiefly Italy and France. The translation has been done well, and the coverage is thorough. Among the subjects is the history of diving, without and with apparatus, from the first man who ducked his head beneath the surface to Cousteau and Gagnan, with their modern, self-contained diving gear. Special submarine pursuits such as archeology and photography are also considered.

Throughout the book there is evident an attempt to keep the actual hunting and killing of fish in reasonable perspective and to stress the acquisition of scientific facts of behavior and life histories. This is so obviously desirable an approach that the author's emphasis is hardly needed. The matter is well summed up in a brief quotation from J. M. Peres: "Yes, it is interesting to dive deeply, of course, but we are still so ignorant of what happens between zero and thirty feet! We should begin by making a meticulous study of this superficial coastal zone; there alone there is plenty of work ahead of us for a long time to come."

As an example of the thoroughness of treatment we may take the section on venomous organisms. This is divided into coelenterates (with the accent on medusae), echinoderms, and fishes. The creatures themselves, their method of inflicting injury, the symptoms, relative danger, and subsequent treatment are discussed in detail.

After an account of the habits of sharks and the relative danger of their attack, the author ends with a quotation from Cousteau: "It is impossible to guess how a shark will react; the more one sees of them, the less one knows them, and one mistrusts them more and more. By constantly worrying them in their own world, divers are asking for trouble. I am expecting to hear of an accident any day now, and it certainly won't be long before one happens."

In the course of hundreds of dives I have never been attacked and only rarely threatened, but I heartily concur in the final words of the author—"Beware of the sharks!"

On the whole, the volume is a welcome improvement over the majority of the numerous books on skin diving. It is well balanced, clearly written, and worth being read by those who are desirous of descending beneath the waters and those who are curious to learn of the apparatus by which man has made this possible.

The reader is grateful for an abundance of line cuts and for adequate indices, one of scientific names.

WILLIAM BEEBE
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New York Zoological Society,
Trinidad, British West Indies

The Space Child's Mother Goose. Verses by Frederick Winsor. Illustrations by Marian Parry. Simon and Schuster, New York, 1958. 45 verses and a glossary of terms. $3.50.

A little knowledge of the vocabulary of science and technology plus an unatrophied delight in *Mother Goose* are positive indicators of a pleasant encounter with the space child's version of a number of old favorites.

The first verse gives the flavor of the volume.

> Probable-Possible, my black hen,
> She lays eggs in the Relative When.
> She doesn't lay eggs in the Positive Now
> Because she's unable to Postulate How.

Or perhaps you will like better

> *Möglich-Warscheinlich, mein' Schwartzhenn',*
> *Legt ihr Ei in das Relativwenn.*

More examples: the mouse on the Möbious strip opens up possibilities unknown to the mouse on the clock; anyone who has ever bounced a youngster on his knee to "This is the way the

farmer rides" will know exactly how to show a child that "This is the way the Rocketman rides: JATO! JATO!! JATO!!!"; one can puzzle over the affairs of the three jolly sailors who "went to sea in a bottle by Klein"; and one can continue for quite a while after learning that "This is the Flaw that lay in the Theory Jack built."

I could go on and on, but
Russell and Whitehead and Hegel
and Kant!
Maybe I shall and maybe I shan't.

DAEL WOLFLE
American Association for the Advancement of Science

The Liberal Arts College. A chapter in American cultural history. George P. Schmidt. Rutgers University Press, New Brunswick, N.J., 1957. ix + 310 pp. $6.

George P. Schmidt of Douglass College, Rutgers University, has accomplished a feat as unusual as it is useful. In 264 pages of text he has compressed a thoroughly readable and remarkably well-balanced account of the history and present situation of the liberal arts college in this country. The 31 pages of notes, together with the brief bibliographical note, provide a well-selected list of authorities and statistics on all manner of colleges and situations. Here, for example, we can find that in 1830 the essential expenses of attending college varied from a minimum of $72 a year (at Hamilton) to an estimated maximum of $190 at Yale. Or that in 1860 Yale had the largest student body (502) but a smaller faculty and smaller library than Harvard, the latter having a faculty of 24 and a library of 123,400 volumes for 409 students against Yale's 21 and 67,000. But the citation of such statistics may give the impression that this is a compendium of facts and figures. It is not that. Schmidt has written a short account of the rise of liberal arts colleges which makes skillful use of figures, frequently in notes, only to round out and support his story.

The book opens with a chapter on the "first fruits," a chapter on "Colleges in the wilderness." From the founding of Harvard in 1636 through the establishment of the other pre-Revolutionary colleges, he goes, by easy transition, to the establishment of numerous colleges, many short-lived, in the newer areas. Chapters dealing with the influence of the churches on the establishment and support of colleges, and on the classical tradition, carry the story well into the 19th century. There is a delightful chapter on the "Old-time college president," one which can be criticized only for the restraint exercised by the author—an authority on this subject—in not making it longer. The rise of the women's colleges to an academic equality with those for men, and the emergence of the modern, complex university, in which the liberal arts college is often a minor part, are informative but never heavy.

In the last five chapters, which make up just over a third of the book, the author deals with recent and contemporary tendencies, controversies, and problems. It seemed to me that he exaggerated the importance—for colleges as distinguished from elementary and secondary schools—of "Dewey vs. Hutchins," but he does justice to the influence of the new courses and curriculum of Columbia College and gives some indication of the variety of discussion and of experimentation in recent years.

A book of such broad scope and of so few pages cannot possibly deal fully with any of the scores of colleges, individuals, or arguments which have marked the history and present situation of American colleges. As I read it, I noticed half a dozen or more points, where, as I knew from observation or reading, Schmidt's account was incomplete, perhaps misleading. But it is not offered as a detailed, much less as a definitive, history. Within its limits of space it is remarkably comprehensive and well documented, as well as clear, honest, and entertaining. What more could we ask for?

BENJAMIN F. WRIGHT
Smith College

The Immense Journey. Loren Eiseley. Random House, New York, 1957. 210 pp. $3.50.

Loren Eiseley has taken an immense journey through time: backward to catch tantalizing glimpses through the fog of our ignorance at the beginnings of things; forward to wonder, a little pessimistically, about the future of man. It is an imaginative journey, reported in an imaginative and evocative prose. Eiseley is not trying to popularize science. He is writing about his own inward experiences, about his reactions to the paleontological record, about his wonderment at the world in which he finds himself. He is writing for the love of words and metaphors and ideas—I wonder whether love of words and of ideas can be separated—and he should be judged in terms of this intent.

I suppose scientists can be roughly divided into two groups: those mostly impressed by our knowledge and those mostly impressed by our ignorance. The "look-at-how-much-we-know" people will not like Eiseley's book. The others will find the reading a pleasant and rewarding experience, often provocative, often stimulating, often esthetically satisfying because of the aptness and beauty of the phrasing.

Eiseley is professionally concerned with human evolution, and much of *The Immense Journey* turns on this problem. Some of the text, on the break between Darwin and Wallace, was published in *Harper's Magazine* a couple of years ago. At that time Eiseley was criticized by some biologists for "undermining public confidence in evolution" or something similarly absurd. Because he explained Wallace's views and doubts, it was felt that he shared Wallace's mysticism. It seems to me, on the other hand, that Eiseley is looking at man in a quite hard-headed fashion, because he is willing to sketch problems for which he has no present and sure solution. We are not going to find the answers in human evolution until we have framed the right questions, and the questions are difficult because they involve both body and mind, physique and culture—tools and symbols as well as cerebral configurations. These are now the separate problems of many different sciences, but the understanding is a single problem for all science.

MARSTON BATES
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Engineering Manpower, How to Improve its Productivity. A special report for management by graduate students at the Graduate School of Business Administration, Harvard University, Boston, 1957. Engineering Managements Reports, P.O. Box 161, Cambridge, Mass., 1957. 162 pp. $16.50.

This is a report of a comprehensive survey of management practices in companies where engineering is an important function. It is the work of nine graduate students under the direction of Georges F. Doriot. They are R. E. S. Arndt, J. L. Clark, C. W. Coker, Jr., W. B. Ellis, Jr., G. C. Hibben, C. B. Johnson, Jr., S. Keehn, R. M. Pribleau, and G. A. Peterfly, the group leader.

The report concludes that the modern management practices that have been so effective in sales, production, finance, and other departments have not been applied so thoroughly in engineering. And here is something that can improve the effectiveness of engineering departments. Part of the trouble has been due to top management's not understanding the engineer's problems. Great improvement should be made in the engineering departments. In particular, the authors point to the need for better planning and leadership. Engineering's first-line supervisory personnel should be trained

in human relations, communications, and management practices, such as the setting of objectives, work evaluation, control, and appraisal.

The survey was made through a large number of personal interviews and correspondence. Questionnaires were also used to get wider coverage. The report is presented under 12 headings: organization, planning, hiring, stock-piling–hoarding, training, placement, leadership, supporting personnel, physical working conditions, personnel administration, compensation, and patents.

The summary on compensation is the least topical. It deals particularly with the narrowing range of engineers' salaries. The reduced spread for experience (telescoping) is considered serious. At the same time, the widening spread between salaries of engineers and of management seems to be condoned. For some time, even before the sputnik, leading companies have been concerned about the narrowing range of engineers' salaries for both experience and merit. Something has been done about it. A comparison of the Engineers Joint Council salary surveys for 1952 and 1956 shows that the tide has turned. The merit spread is being increased in some companies by providing two "ladders" for engineers to climb to the highest salaries—one the management ladder, the other the professional competence ladder.

This report presents a good case for more modern management practices in engineering. It is good reading for engineers and executives.

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UNESCO. Purpose, progress, prospects. Walter H. C. Laves and Charles A. Thomson. Indiana University Press, Bloomington, 1957. 469 pp. $7.50.

It may be that, in the years to come, somebody will write an "official" account of the birth and infancy of the United Nations Educational, Scientific and Cultural Organization (UNESCO), but, if so, it will not replace this book. Laves and Thomson have had access to more than the facts and documents. They have breathed the atmosphere of UNESCO's "founding fathers," and they know well the early hopes and plans. They themselves helped to shape the course of this *enfant terrible* of the U.N. family.

As a result, the book UNESCO minds its three *p*'s very well. It rejects the sentimentalist who, had he prevailed, would have made of UNESCO a pink cloud floating over and beyond all problems of practical significance. The authors have produced a reference work for the student, the teacher, or the government official. The 100 pages of notes at the end are invaluable source material.

The authors give a brief appraisal of UNESCO's achievements to date. The purpose of the organization is held to be sound in principle, even if somewhat diminished in practice. The unpredicted cold war inevitably relegated the United Nations' specialized branches to the business of reconstruction, technical assistance, and various forms of cultural interchange. Deprived of any leading role in keeping the peace, UNESCO, through various conferences and publications, has managed to study and report upon the great issues and to free the channels of communication. Increasingly it has recognized the political character of its basic decisions.

Progress in UNESCO has been twofold: (i) improvement in its educational, scientific, and cultural programs (through more concentration and better planning) and (ii) the slow realization that UNESCO could be effective if the member states would bring it into the center of the planning for peace. Perhaps a tie in the race for hydrogen-headed monsters will turn the nations toward the needs shared by all humanity. In this sense, UNESCO, an organization devoted to ideas, values, and humane services, is not so much a constructor of peace as it is a living example of whatever good will exists among the nations.

Laves and Thomson make no attempt to arouse support for UNESCO. Their style is quiet and judicial, although the final note is optimistic. The volume is useful and informative, but it does not dig deep, and it will stir no one to action. What UNESCO needs now is some penetrating writings that carry a sense of urgency.

GEORGE D. STODDARD
New York University

Conservation. An American story of conflict and accomplishment. David Cushman Coyle. Rutgers University Press, New Brunswick, N.J., 1957. xii + 284 pp. Plates. $5.

David Coyle has given us what is probably the first short but adequate history of the conservation movement in the United States. It offers the reader a comprehensive understanding of the principles of conservation, the development of the various branches, and the conflicts that arose in the many public issues that were involved in the movement. The subject is presented in four parts, with 16 chapters.

Part I, entitled "The beginnings of conservation," presents an excellent historical account of conservation. It is concerned chiefly with conservation of our forest resources. The meeting of prophet Gifford Pinchot with President Theodore Roosevelt marked the beginning of the conservation movement in the United States. The Forest Service grew up from the old Division of Forestry, which had a two-room office and a total staff of 11 people. Our present knowledge of forestry techniques has grown from Pinchot's *Primer of Forestry*, which told enough about the woods to guide the beginning forester or lumberman.

In part II, entitled "Reaction, war, and normalcy," the policies of conservation built up by Pinchot and Roosevelt are discussed; these came into direct conflict with the old-fashioned ideas of Taft and Ballinger. After this battle, the movement developed quietly for 25 years, until the next outburst in the expansion of 1933.

Part III deals with events of 1933 and after. Great strides were made in all branches of conservation under the leadership of President Franklin D. Roosevelt. Forestry marched forward under the impetus of the Civilian Conservation Corps program, the Norris-Doxey Law, the Taylor Grazing Act, and the shelterbelt program. This era saw the permanent establishment of a definite program of soil conservation. Progress was made on the problems of water, floods, navigation, irrigation, water power, rural electrification, and wildlife conservation. Such matters as Dixon-Yates and Hell's Canyon are discussed.

This well-organized and clearly written story of conservation could very possibly be used as a textbook for courses in forestry, conservation, wildlife, and agriculture.

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Advances in Documentation and Library Science. vol. I, *Progress Report in Chemical Literature Retrieval.* Gilbert L. Peakes, Allen Kent, and James W. Perry, Eds. vol. II, *Information Systems in Documentation.* J. H. Shera, Allen Kent, and James W. Perry, Eds. Interscience, New York, 1957. 217 pp.; 639 pp. $4.75; $12.

The first two volumes of *Advances in Documentation and Library Science* mark the beginning of a new and potentially useful undertaking. Two of the avowed purposes of this undertaking are "To meet the need for a publication the pages of which will be available for the recording of conference proceedings," and "To provide an instrument for the publication of monographic materials which are either too long or excessively detailed or specialized for the existing journals." These are laudable purposes.

Since documentation—and particularly scientific documentation—is an amalgamation of many disciplines and activities, it is often difficult to find a suitable forum for publication of the deliberations of documentation experts.

The first volume of *Advances in Documentation and Library Science* is entitled, *Progress Report in Chemical Literature Retrieving*. It combines the proceedings of two American Chemical Society symposia on the indexing of scientific literature, and it does so in such a way that it provides the reader with a bird's-eye view of past, present, and future developments within the field. It is informative without being verbose, saying what it has to say directly and economically. Volume I constitutes, in my opinion, a well-balanced, extremely informative text on the principles of indexing which should satisfy both the neophytes and the sophisticates.

Unfortunately, the promise of volume I is not fulfilled in volume II, which is entitled *Information Systems in Documentation*. Volume II is the proceedings of a symposium on systems for information retrieval, which was held at Western Reserve University in 1957. To judge from its program, this symposium must have resembled a three-ring circus. Volume II also takes on many of the characteristics of a three-ring circus. It contains so much material that it is overpowering. In their attempt to give comprehensive coverage to current activities in the organization of information, the editors intersperse many second-rate papers among many first-rate papers. This is a waste; it merely makes the good papers hard to find, and it forces the reader to plough through a maze of inferior papers that should not have been published in the first place.

The contrast between the first and second volumes of *Advances in Documentation and Library Science* illustrates a problem which is a common one in the publication of proceedings. Are published proceedings truly proceedings if the editors exercise judgment and leave out papers which were presented but which are not worthy of publication? The editors of the second volume apparently decided in the negative. It is to be hoped that the editors of future volumes of *Advances in Documentation and Library Science* will take an opposite stand.

From the word *Advances* in its title, one is at first given the impression that *Advances in Documentation and Library Science* is a review publication in the classical, critical sense. *Advances in Documentation and Library Science* is not, at this stage of its development, a true review publication. It would be a good thing if it should become one.

SAUL HERNER

Herner and Company,
Washington, D.C.

Biology and Medicine

A History of Nutrition. The sequence of ideas in nutrition investigations. Elmer Verner McCollum. Houghton Mifflin, Boston, 1957. x + 451 pp. $6.

It is given to few persons in each generation to make history and to be able to write history. With the publishing of his *History of Nutrition*, E. V. McCollum has joined the small but distinguished company of doers who are also historians. The scientific fame of McCollum is secure: It was he who clearly demonstrated the existence of fat-soluble vitamins and differentiated vitamin A from vitamin D, among other major achievements. He is thus particularly well qualified to deal with the development of our knowledge of dietary essentials.

In his historical venture McCollum traces the development of what Lusk called "the Science of Nutrition" from its empirical beginnings to the early 1940's. His chapter on ancient—Greek and Roman—concepts is particularly interesting. McCollum deals briefly with the 18th century; while his descriptions of the work of the great innovators of this era are interesting, he could, perhaps, have underscored more emphatically the profound revolution in thought brought about by Lavoisier's equating the oxidation of food with physical phenomena reproducible in a calorimeter. McCollum's treatment of the application of the new chemical techniques to problems of food composition during the 19th century is particularly well done; the influence of Liebig is justly emphasized. The birth of the concept of qualitative requirements with Magendie and the pioneer role of Boussingault in the development of agricultural chemistry, plant nutrition, and the nutrition of farm animals are given the place their merit earns.

The second part of the book, which deals with the evolution of knowledge of dietary essentials, is particularly useful: McCollum deals with the development of general concepts, with particular emphasis on vitamins (amino acids are perhaps somewhat slighted; minerals are adequately dealt with), then goes on to tell the story of each of the principal vitamins. I was especially interested in the relation of events and discoveries in which McCollum himself took part. His personal recollections and appraisals of contemporary reactions to various findings and new ideas are particularly fascinating. McCollum's very candid account of some of his initial errors and of how he progressively corrected them under the presence of accumulating evidence is a superb lesson on the experimental method.

McCollum ends his book with the year 1940, prior to the explosive development of international and national work in "applied" nutrition. One may wonder why in his last chapter, "The end of an era: New horizons," he did not at least mention such milestones of the 1930's as the first set of nutritional requirements (proposed by the Physiological Committee of the League of Nations, in which McCollum himself took a leading part), the pioneer report of Aykroyd and Burnet on the feeding of populations, that of Bigwood on assessment of the nutritional status, the first large-scale surveys (by John Boyd Orr in Scotland and by Stiebeling and Phipard in the United States), and the recommendations of the Mixed Committee of the League of Nations, which gave the philosophical basis for the creation, later, of such institutions as the Food and Agriculture Organization of the United Nations. At a time when the influence of scientific research on the human community is not an unmixed blessing, it might be worth recording that, perhaps alone among the natural sciences, nutrition has been consistently on the side of the angels.

It is unfortunate that the publishers did not give this book better editorial attention. Foreign references ought to have been more carefully checked. Some proper names are misspelled in the text; there are a number of different abbreviations for the same journals in the bibliography, and many of them are incorrect. This is particularly evident for French references, but German and Italian references are not immune to errors. It is hoped that these mistakes will be corrected when this important and highly useful book is reprinted.

JEAN MAYER

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Vitamin A. Thomas Moore. Elsevier, Amsterdam, 1957 (order from Van Nostrand, Princeton, N.J.). xx + 645 pp. Illus. $14.

This volume is remarkable for its comprehensiveness, combined with perspective. Critical judgment of the work of others is not avoided but is always kindly. Attention has been given especially to recognition of those who were first to publish each feature treated. The organization of the entire work will be most helpful for the various types of readers who will desire to use this volume as a reference source concerning the several forms of vitamin A and the carotenes.

This comprehensiveness and perspective have been made possible in large part by the fact that the author, the deputy director of the Dunn Nutritional Laboratory, Cambridge, England, has

participated in many different aspects of the ... of vitamin A over a professional lifetime of nearly thirty years. He has obviously been as systematic in his reading and recording of bibliographic data as he has been in conduct of his own research.

The comprehensive character of the work is shown by the sectional organization of historical, assay, chemical, biochemical, physiological, pathological, clinical, dietetic, and animal-husbandry material. In many cases data of importance may be found in two or more of these sections, but with cross reference. This makes the volume useful for quick reference without the necessity for consecutive reading of the entire book. The author shows a gratifying perspective about the various facets of the highly complicated problems he treats.

As features of the excellent organization of this book, mention should be made of the adequate summaries which introduce chapters and sections of the volume. Liberal use of marginal topical headings facilitates rapid perusal of long chapters. The documentation is organized by chapters and is very extensive but is limited to items used in development of the themes.

The line drawings are well done. Some of the plates are reproductions of the classics in the field, suffering as usual from the technical faults inherent in such copying. Nevertheless, they are useful in this compendium of the progress in our knowledge of vitamin A, to date.

The appendix, with nine sections, provides much technical information, which has been well organized. The indexes are separate for authors and subjects. Both are very complete. It is notable that all the authors of each paper quoted are to be found separately listed.

This book will be found useful in the libraries of a wide variety of those interested in vitamin A. Among other matters it provides numerous suggestions of the gaps in the total picture, where investigators might well undertake further research.

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German-English Glossary of Neurophysiology. Roger Merritt Morrell, Ed. Consultants Bureau, New York. 181 pp. $7.50.

This German-English dictionary of the specialized language of physiology, anatomy, biochemistry, and electronics was assembled by an American neurophysiologist to help him in his work. Some 9000 words and phrases are entered, these being translated, mostly, into single English words or phrases. The book contains many misspelled German words and incorrect translations, there being at least 20 such, for example, between pages 58 and 61. The book is very cheaply—and in places very badly—reproduced from a typescript, and it is difficult to understand why it should be so expensive. On the positive side, elimination of low-priority information about the German words (for example, their gender, and umlauts) makes the list easy to use. This book may, despite its many faults, prove to be of some value to English-speaking specialists with a limited knowledge of German scientific terminology.

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Zoogeography: The Geographical Distribution of Animals. Philip J. Darlington, Jr. Wiley, New York, 1957. xi + 675 pp. Illus. $15.

The orderly geographic patterns of systematic groupings of animals have long challenged the interpretive mind. With modern progress in geology, systematic zoology, ecology, phylogeny, and evolutionary theory, the time is ripe for correlating the data from different fields of biology into a geographic synthesis. Darlington has written an admirable book with a critical evaluation of facts and principles of the distribution of land and fresh-water families of vertebrates. Geographic order and its interpretation are of value not only to biogeographers but also to taxonomists, ecologists, and evolutionists. Principles deduced from the distribution of the lower taxonomic categories are less adequately treated than are those for the families and higher categories, but *Systematics and the Origin of Species,* by E. Mayr (Columbia University Press, 1942), fills this gap. It is understandable why the invertebrates were left out. The systematics, ranges, fossil histories, and phylogenies of the vertebrates are much better known. However, a few excellent studies of the distribution of invertebrates have been published. *Evolution and Classification of the Mountain Caddisflies,* by H. H. Ross (University of Illinois Press, 1956), *The Faunal Connections between Europe and North America,* by C. H. Lindroth (Wiley, 1957), and Darlington's own studies of the Carabid beetles might have been used for corroborative evidence for some of the main concepts.

Many of the gaps in Darlington's book are not the fault of the author. The fault is rather in the complexity of the subject, the dependence upon an unattained taxonomic and phylogenetic precision, and the prevalence of errors in the literature that can only be corrected by specialists (of which there are far too few) using modern systematic techniques. Darlington has gathered much evidence for the contemporary and ancient causes of major geographic patterns, and even with the omissions, the book should be an important reference work for many years to come.

The style is rather individualistic and, in my opinion, makes for easy reading. Dogmatism is avoided, sometimes to the point of obscuring the evidence that has statistical significance. The illustrations are highly diagrammatic and simplified. More detailed figures of vegetation and climatic patterns would have clarified the correlated animal distributions. For example, Fig. 50 could have shown some of the main ecological features of the Nearctic region, but, as it stands, it is a waste of space. Phylogenetic trees, with inserted data on fossils, geologic time, and contemporary distribution in zoogeographical regions, would have clarified many of the discussions in the text.

I commend the author on his useful tables and lists, upon his use of fossils, and upon his discussions of controversial concepts such as Wegnerian continental drift, oceanic land bridges, and the origin of island faunas. The book is a first-class text for a course in zoogeography, particularly if *Ecological Animal Geography* by Hesse, Allee, and Schmidt (Wiley, 1951) is used as a companion volume.

Much useful zoogeographical information still remains to be discovered, but Darlington has provided a base for further exploration. There is no question but that the science of animal distribution is still in its infancy, but great promise for future maturity is already evident.

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The Friendly Fungi. A new approach to the eelworm problem. C. L. Duddington. Faber and Faber, London, 1957 (order from Macmillan, New York). 188 pp. Illus. $4.50.

The Friendly Fungi is concerned with the exciting possibility of using predacious fungi for the biological control of eelworms (nematodes) which beset cultivated plants and domesticated animals. These fungi are "friendly" only from our point of view, because the way in which they capture eelworms by means of garrotes, snares, adhesive pegs, and sticky nets and the efficiency with which they dispatch their prey make them distinctly unfriendly to nematodes.

The book begins with a description of nematodes, their habits, and their economic importance. It then goes on to a consideration of the methods by which the control of these pests has been attempted and the difficulties which have

been encountered. The next chapters concern the fabulous predacious fungi themselves and review some of the investigations which have been conducted to determine the distribution and mode of operation of these extraordinary organisms. The possible usefulness of these fungi for the biological control of nematodes becomes apparent during these chapters, and the last part of the book is devoted to an account of the limited attempts that have been made to apply these capabilities and to suggestions for future work.

After reading the description of them in this book, many readers will be tempted to have a look at predacious fungi for themselves, and yielding to this temptation is facilitated by two appendixes. One appendix gives methods for obtaining and examining predacious fungi for those who have at their disposal the usual equipment for dealing with microorganisms; the other gives improvised methods which can be used by anyone who has a microscope as his only piece of scientific equipment.

The book should appeal to anyone who is interested in living things. Not only are the predacious fungi of great inherent interest, but the ecological problems touched upon and, indeed, the whole matter of biological control are of very general concern. Moreover, the book is not written for the specialist; in fact, its style is an excellent example of semipopular scientific writing, because it is straightforward and avoids unnecessary terminology without sacrificing accuracy or being condescending. It is to be hoped that the book will find its way not only to the shelves of mycologists but also into the hands of high-school students and college undergraduates who are casting about for reading to supplement a course or for a subject for a research project, because such students will find *The Friendly Fungi* instructive and stimulating, just as the specialist will find it delightful.

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The Life of Mammals. J. Z. Young. Oxford University Press, New York, 1957. xv + 820 pp. Illus. $10.

J. Z. Young has already devoted 200 pages to the origin, evolution, classification, and ways of life of mammals in his *The Life of Vertebrates* (1950). This companion volume was constructed from the essentials of mammalian gross anatomy, histology, physiology, and embryology, smoothly fashioned into an account of what it takes to keep mammals alive as individuals and to perpetuate them as species. It is intended as a text for premedical students, but it could be read with enjoyment by any biologist and deserves wide notice as an example of a relatively new pattern of thinking about living matter.

Biology, says Young, must free itself from the limitations of an outmoded language. The uniquely biological properties of living matter cannot be described properly in terms now available, but as a step in the right direction he suggests the developing language of information theory. He sprinkles his text with terms like *feedback, servomechanisms, control systems, signals, sets of instructions, coding and representation.*

A mammal (or an ameba) is distinct from its environment yet continually makes interchanges with it and can survive only by containing within itself stores of information which represent what its environment has been in the past, and through which it can calculate what the environment will probably be in the future. Then it must be patterned so that it will select one of the alternatives of behavior which will be permitted by the environment. Part of the store of information (for which the word *memory* is freely used) is printed into the genes, the rest being garnered and stored here and there during the life of the individual. All cells, all tissues, exist in a state of "double dependence," becoming patterned or controlled by these two stores of information, whether in the maintenance of a homeostatic economy or in the production of a new one. It is urged throughout the book that such a choice of metaphor is unifying and fruitful in the coordinating of knowledge of such different disciplines as gross anatomy and biochemistry. Young suggests that suitable terms for describing forebrain functions will presumably evolve as men make machines that imitate these complicated actions more closely.

However, this is far from being a tract for philosophers. The gross anatomy of mammals is covered in detail quite sufficient for the premedical student, the rabbit being chosen as type. Skeleton, muscles, and nerves are considered as functional units for support and locomotion. There are solid traditional approaches to the subjects of digestion, respiration, endocrinology, and so on. The concept of homeostasis is examined from many angles. Chapters are devoted to the body fluids, to the sensory systems, to the functions of the central nervous system, and to recent advances in muscle and nerve physiology. Fourteen of the 47 chapters deal with embryology, and six of these constitute the principal departure from mammalian biology, presenting experimental embryology pertaining to amphibians and birds.

To the credit of the author and his style, and by virtue of his selection from what, even to him, must have seemed an endless range of available knowledge, there comes out clearly in this book a sense of the staggering intricacy, and the almost unbearably beautiful fitness, of the parts of living things.

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The Physiology of Fishes. vol. II, *Behavior.* Margaret E. Brown, Ed. Academic Press, New York, 1957. xi + 526 pp. Illus. $14.

Volume I (*Metabolism*) of *The Physiology of Fishes* was reviewed in the 8 Nov. 1957 issue of *Science* [**126**, 981 (1957)]. Happily, only a short interval elapsed before the appearance of volume II (*Behavior*). Perhaps it is a sufficient commentary on the state of our knowledge of fish physiology that the second volume is the larger of the two and, in that sense, represents a triumph for the students of behavior. So important has their use of fishes as experimental animals become, and so much does it promise, that whole fields of classical physiology may be thought of as tools for the study of behavior.

Treatment of these tools makes up about 60 percent of the volume, the largest single section being that on the nervous system, by E. G. Healey. The group of chapters on conditioning (H. O. Bull), ethology (G. P. Baerends), and reproductive behavior (L. R. Aronson) constitutes less than another 20 percent, as do the chapters on the eye (J. R. Brett), the acousticolateralis system (O. Lowenstein), and the olfactory and gustatory senses (A. D. Hasler).

To this core is added a group of chapters on the structures that are unique, or almost so, to the fish: the swimbladder, by F. R. Harden Jones; the electric organs, by R. D. Keynes; and the luminous organs by E. Newton Harvey. Each of these authors is able to present a considerable body of work published since 1950. The next two very brief chapters, on the pigments (D. L. Fox) and color changes (J. M. Odiorne), present relatively little new work but draw together the material relative to fish. The chapter on the quality of water required by fish, by P. Doudoroff, might almost be entitled "pitfalls in conducting experiments with fish." The book concludes with a useful review of the physiological genetics of fish, by M. Gordon.

The choice of contributors to volume I was rated as outstanding; the high standard has certainly been maintained in volume II. With minor exceptions, each chapter was contributed by an active worker, usually one whose own contributions to the field in question have

been noted. Only through such joint authorship could so authoritative a volume have been produced. There is no common index, and cross references are few. Nevertheless, owners of volume I will certainly wish to own volume II, and members of the new group who have been led, because of special interests, to purchase volume II, will soon find a strong compulsion to make their sets complete.

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Veterinary Toxicology. Formerly *Lander's Veterinary Toxicology*. R. J. Garner. Bailliere, Tindall and Cox, London, 1957 (order from Williams & Wilkins, Baltimore). 415 pp. $7.50.

This unillustrated book deals with a study of the origin, properties, and effects of poisons, with their detection, and with the treatment of their effects. This is a complete revision of the third edition of *Lander's Veterinary Toxicology*, as revised by Nicholson, which was published in 1945. The new author, Garner, refers to Nicholson very frequently in the voluminous references, for which, as the book shows, Garner has done an enormous amount of research. Much of the original text has been rewritten and rearranged. The botanical description and illustrations of plants have been deleted from the section on poisonous plants. Newer subjects, such as chemical pesticides and radioactive substances, have been added and given proper consideration and importance by the devotion of a section to each. The data on chemical analysis for poisons have been brought together in another section.

There is much documented evidence, with references, to support what the author says, and it is well arranged. There is a good index, which makes the book valuable as a ready reference. Although considerable space is very appropriately devoted to the toxicology of both chlorinated hydrocarbon and organic phosphorus insecticides, it does not seem as though the list of references is correspondingly long, and I believe that the U.S. Department of Agriculture's *Technical Bulletin 1122* on these subjects is worthy of reference. In the section on poisonous plants there is no mention of greasewood and halogeton, two oxalic acid plants, or of milkweed and locoweed, all of which have received considerable publicity in American literature. There also is little information given about a number of poisonous plants important in the United States, such as larkspur, arrow grass, fescue, and astragalus. However, the book brings together a wealth of information, arranged in such a way that it is easy to find. Moreover, a book on this subject has been urgently needed. In the changing field of agricultural chemicals, with new chemicals reaching the farms, the animals, and the animal feeds, veterinary toxicological problems may appear or be suspected in connection with numerous agricultural practices. Veterinarians, research workers, students, and others interested in toxicology will find this book of value.

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On the Utility of Medical History. Monograph I, Institute on Social and Historical Medicine, New York Academy of Medicine. Iago Galdston, Ed. International Universities Press, New York, 1957. ix + 73 pp. $2.

Iago Galdston, one of the country's most articulate promoters of the writing of medical history, has written or edited many volumes dealing with current medical issues in which the historical background is fully set forth because he believes that medical history, by illuminating the present, provides a basis for judgment about the future. To inquire whether this belief is well founded and, if it is, how the potential utility of medical history may be more fully exploited, he organized the conference or institute reported in this book. Other contributors besides himself are George Rosen, Owsei Temkin, Gregory Zilboorg, Erwin H. Ackerknecht, and Paul Schrecker. In a series of essays they define and discuss purposes, values, goals, and uses of medical historiography, teaching of medical history, and medical history itself.

After finishing the essays, my first reaction was to write a review article on the utility of medical history, exploring the distinction between the *value* of history studied for itself and the *utility* of history as an aid to solving current problems—reasserting the cultural and humane values in history which the essayists seem reluctant to expound and urging medical historians in search of self-justification to admit their interest in the subject for its own sake and to say less of the utility of medical history, which cannot be proved, lest they promise more than they can produce. Lacking space, I set my essay aside unfinished. But the reader may judge from this how thought-provoking I found the book.

Although the uses of history in general have been discussed more fully by a number of other historians, and although earlier articles by Temkin, Rosen, and Ackerknecht on the usefulness of medical history and its role in medical education are as good as if not better than their contributions here, anyone who is particularly interested in medical history may gain new insights from these stimulating essays. The book's chief value, however, will be realized if it is able to reach and influence the unconverted who can profit from it most. If it does, it will have performed a useful function.

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Metabolic Aspects of Transport Across Cell Membranes. Q. R. Murphy, Ed. University of Wisconsin Press, Madison, 1957. xxiv + 379 pp. Illus. $7.50.

Movement of substances across the boundaries of living cells is obviously necessary for their continued existence. In recent times it has become apparent that some substances do not always move simply down concentration gradients or gradients of chemical or electrochemical potential; that there are net movements against such gradients is now indeed a well-established fact. Elucidation of the mechanism underlying this type of transport and the way in which it is coupled to required energy sources is the current goal of much physiological research and is the central topic of this book.

The volume contains the reports and discussions of a symposium held at the University of Wisconsin in August 1955. Brief specific mention of the principal topics will indicate the scope of the subject matter: regulation in fluid exchange (E. F. Adolph); ion exchange in clinical medicine (D. C. Darrow); principles and theories of transport (H. H. Ussing); movement of water and electrolyte across intestinal epithelia (M. B. Visscher); transport of ions in skeletal muscle (E. J. Conway), in cardiac tissues (S. Weidmann), in vertebrate nerve (A. M. Shanes), in renal tubules (R. W. Berliner); gastric acid secretion—the present position (R. E. Davies); gastric acid secretion—metabolic aspects (H. W. Davenport); and gastric acid secretion—compatibility of theories with electrophysiological findings (W. S. Rehm and W. H. Dennis). Additional brief reports preface each discussion.

Areas of discussion are: (i) ion transport in muscle and nerve, (ii) ion transport across renal epithelium, (iii) transport of organic compounds across renal epithelium, (iv) gastric acid secretion, and (v) correlation of clinical problems and basic information relating to membrane transport. Transport in plant cells is not considered.

Although many of the observations described here have already been published elsewhere and although several of the principal speakers have outlined their views in other recent symposia, the book remains fresh and valuable on the basis of the discussions alone. Provocative speculations are plentiful, but so are earnest and pertinent warnings about experimental procedures and hidden assumptions. The discussions emphasize further the diversity of current opinion as to the nature and sequence of molecular events that permit cells to maintain their ionic steady states or to function as secretory units.

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Biochemie der Ernährung. K. Lang. Steinkopff, Darmstadt, Germany, 1957. xv + 411 pp. Illus. DM. 54.

Many futile attempts have been made to expound the subject of nutrition with a minimum regard for chemistry. This volume certainly does not belong in this class. It is a worthy and very substantial book which treats in an excellent and thorough manner the chemistry of many food constituents. The treatment of carbohydrate chemistry (9 pages), the chemistry of fats (17 pages), and protein chemistry (50 pages) is less elementary than might be supposed but not, in general, as adequate as is the treatment of the vitamins, which occupies approximately one-half of the volume and has to do not only with the chemistry of the vitamins themselves but with their functional derivatives. The lack of attention to carbohydrate chemistry, for example, may be justified on the grounds that other books dealing with this subject are available and advances in the carbohydrate field do not impinge very directly on current advances in nutrition. There are about 650 excellent references cited in the volume, and it constitutes a sound piece of work.

In recent years it has become evident that nutrition cannot be treated in a thoroughgoing manner without reference to genetics. It is clear that genetic variations cause different species, strains, and individuals to have differing nutritional needs. This book has, from my standpoint, a serious fault in that the relationship of biochemical genetics to nutrition is not discussed. The time has already come, in my opinion, when the biochemistry of nutrition cannot be treated in a scholarly way with a disregard of the role that genetics plays.

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History and Philosophy of Science

Copernicus. The founder of modern astronomy. Angus Armitage. Yoseloff, New York, 1957. 236 pp. Illus. + plates. $5.

Many American readers already know Angus Armitage as the author of *Sun, Stand Thou Still,* a popular account of the life and work of Nicolaus Copernicus, first issued in 1947 and recently more widely circulated in paperback under the title *The World of Copernicus.* Fewer are aware that that elementary volume was itself the by-product of an earlier and more scholarly study, *Copernicus: The Founder of Modern Astronomy.* Since the earlier volume was and is the only serious and detailed study of Copernicus' astronomical research in English, this revised and expanded edition is very welcome.

Armitage begins his book with a condensed sketch of the development of planetary astronomy in antiquity and the Middle Ages. His second chapter recounts most of what is known of Copernicus' life, the progress of his astronomical research, and the composition of his *De Revolutionibus Orbium Coelestium.* Then follow four chapters dealing in detail with Copernicus' astronomical system. Two closing chapters and an epilog (all added for this edition) discuss the gradual acceptance of the new astronomy, its physical verification, and, very briefly, its subsequent fate. There are also three useful appendices and a brief index.

The five chapters dealing with Copernicus and his work are the heart of the book, and they are uniformly readable, reliable, and illuminating. But three of them—those that discuss Copernicus' treatment of the motion of the earth, the moon, and the planets—are also something more. Readers unwilling or unable to grapple with the complexities of the *De Revolutionibus* itself will find in these chapters the only reasonably full account of Copernicus' real contribution to modern astronomy: a sun-centered, mathematical planetary system able to compete with the earth-centered system of Ptolemy. In the 16th century the concept of a moving earth was not novel. It was the mathematical demonstration that a moving earth was compatible with existing standards for predictive planetary astronomy that gained for Copernicus enduring fame.

But Copernicus gave a strange demonstration. Armitage shows what others have mentioned—the *De Revolutionibus* was modeled, both in organization and in mathematical detail, upon Ptolemy's *Almagest.* To describe the earth's orbital motion about the sun, Copernicus compounded three perfect circular motions; three more were required to describe the moon's motion about the earth; two or more circles (epicycles and eccentrics) were used to trace the motions of each of the planets about the moving center of the earth's orbit. When he was through, Copernicus' system was quite as good as Ptolemy's, but it was neither more accurate nor appreciably simpler. Copernicus' single concrete improvement (one which attracted many later astronomers to his work) was in lunar theory, and to this, ironically enough, the concept of the earth's motion was irrelevant.

Armitage's account of Copernicus' life and of the acceptance of his theory are less unusual. But they are appropriate appendages to his central study, and they are admirably done. The same is not so clearly true of his opening and closing chapters. The first seems far too condensed and simplified to achieve its purpose. Readers can and must find elsewhere both the historical and the conceptual background of Copernicus' work. The last chapter, dealing with the physical verification of the Copernican theory, adds even less to the volume and will very probably mislead. The physical problems raised by Copernicanism are scarcely discussed in earlier parts of the volume. Collected in this summary form, without either historical or technical context, they become almost a parody of the sort of history to which other parts of the book make such notable contributions. But these criticisms are directed only to the frame of Armitage's study. They do not at all reflect on its central chapters. These remain an invaluable aid to all those wishing to learn more about Copernicus and about the astronomical system he designed.

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Science and Human Life. J. A. V. Butler. Basic Books, New York, 1957. 162 pp. $3.95.

The book under review is a work with a philosophical keynote, written by a British scientist whose special field is biophysical chemistry. The author starts from the questions, What are we? What is the basis of human life?, and tries to discover what science, which has revealed so much of the constitution of the universe, can contribute to the answer.

Human beings, who in the prescientific era thought of themselves as the dominant figures in the universe learned, first, that they were only the top members of the earthly animal kingdom, which, in its turn, is an infinitesimally small dwarf among the billions of islands of organic life that may exist on planets of the stars. Second, doubts were even

raised about whether *Homo sapiens* is a rational creature at all, or, as might be inferred from Freud and his school, an irrational one driven by impulses of mainly sexual origin. The author points out that the question whether the scientific findings of our age give a true picture of human nature is not an academic one but is "practical and necessary because, in the last resort, our attitude to all human questions depends on our idea of human nature."

The first section of the book—entitled "Science and the Individual"—reports what modern science has revealed about life as a chemical phenomenon, about the machinery of reproduction and evolution, about the conception of man as an automaton, and about the problem of mind and matter.

Equipped with the necessary stock of scientific knowledge, the reader is led in the second section of the book—"Science and Human Society"—into the field of humanities, where the author turns out to be a humanist himself, crossing the border from science to practical philosophy. While no full and definite answer can be given to the questions put in the introduction, the author explains clearly the great complexity of human nature. Considerations are given thereby to practically important questions such as, for instance, the problem of human values, the lack of correlation between living standard and true happiness, and the responsibility of statesmen in our age in which means of global self-destruction are available.

A bibliography is added to the volume, in which, regrettably, the works by Butler's American colleague, the biochemist Roger Williams, are lacking. Williams' findings of the great differences between human individuals make important contributions to the very subject of Butler's book. Certainly Butler cannot be blamed for having overlooked Williams' work, in view of the vastness of the book market. Still, a closer cooperation between scientists working in the important field of "humanics" is desirable.

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Histoire Générale des Sciences. vol. I, *La Science Antique et Médiévale.* (Des origines à 1450). René Taton, Ed. Presses Universitaires de France, Paris, 1957. 627 pp. Illus.

The first volume of the new series of *Histoire Générale des Sciences,* under the editorship of René Taton, is the most complete and up-to-date survey of the science of these periods in any language. It is of course a collection of studies by experts in the different periods and cultures covered by this volume. But, surprisingly enough, the editor has managed to achieve a stylistic unity that is rather remarkable. As the title indicates, the volume includes science in antiquity and the Middle Ages. It contains three parts. The first part is devoted to pre-Greek science, more specifically, the science of Egypt, Mesopotamia, Phoenicia, Israel, ancient India, and ancient China. The second part is a study of science in the Greco-Roman world, covering, first, the science up to and through the time of Aristotle and, next, Hellenistic and Roman science. The third part includes science of the Middle Ages and, more particularly, that of Islam, India, China, Byzantium, and western Europe.

I think that the best thing about this volume is the fact that it reflects recent research more directly than any other survey volume of which I know. Obviously, with such a broad coverage, it cannot replace some of the standard single-volume studies of shorter periods. For example, the student and reader will still want to consult Neugebauer's brilliant survey of mathematics and astronomy in his *Exact Sciences in Antiquity.* It might be thought at first that the present volume would not completely displace the excellent single volume of Brunet and Mieli covering the ancient period, for that latter work contained a good number of source readings. Still, as the reader examines this volume more closely, and particularly the excellent section on Greek science, he will find woven into the context many fairly long representative passages from the original authors. I think that the reader who is coming to this field fresh, without much previous training, might perhaps do well to read this single volume and to accompany it with a reading of Cohen and Drabkin, *Source Book in Greek Science* —the best single collection of documents on Greek science.

Painfully small and inadequate is the description of the content of Arabic exact sciences. Thus, astronomy is brushed off in only two pages, mechanics and optics are virtually untreated. The chapter on the science of western Europe in the Middle Ages is, in my opinion, very good indeed, although I would like to have seen more space devoted to it (with some of the peripheral material, like that on Phoenician science, Hebraic science, and so on, dropped).

There are, of course, matters of detail I would quarrel about. The author gives the impression that the medieval source of Hero's formula for the area of a triangle in terms of its sides is the *Liber Embadorum* of Savasorda. It had already been stated in the material of the *Agrimensores.* The first time it appeared in Latin *with a proof* was in the *Verba Filiorum* of the Banu Musa, translated by Gerard of Cremona. The medieval section of the volume is quite up-to-date and gives a brief and accurate picture of the main movements in the high and late Middle Ages, but the account of early Latin medieval science, such as it is, is very brief and inadequate.

The volume includes 48 quite handsome plates, and the editor and the Presses Universitaires are to be congratulated on the volume's fine appearance.

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Roots of Scientific Thought. A cultural perspective. Philip P. Wiener and Aaron Noland, Eds. Basic Books, New York, 1957. x + 677 pp. $8.

Aside from prefatory matter, illuminating in its own right, which the editors have provided, the body of this volume consists of 33 papers (by 29 scholars) which first appeared as integral articles in *The Journal of the History of Ideas.* At least a dozen of them are acknowledged as classics in their kind. They have been arranged in roughly chronological order and range in subject matter from early Greek science to recent cosmology, though the heaviest concentration is on the 16th and 17th centuries. There are studies of individual scientists and scientific experiments, of philosophies of science, and of institutions, organizations, and ideas whose influence upon the development of science was more than peripheral. Yet this is not merely an anthology, nor is it only a guide for the antiquarian whose interest happens to be, *per accidens,* the scientific past. For each of the writers is in some measure concerned with the broader lines of interpenetration between science and the other components of culture—philosophy, industry, commerce, religion, and art; and the general theme, which each article has been chosen to illustrate, is that the *idea* of science has cultural roots. To be sure, the idea of science may be in principle distinguished from the positive discoveries and theories of science per se, but the two cannot be held asunder if we wish to comprehend the history of either. No one can read the papers of Moody, Koyré, Randall, and Zilsel and still suppose that the history of science may be written simply as the chronicle of discoveries. But neither can anyone suppose that a simple theory of historical (or cultural) causation can account for these. This double lesson is, perhaps, the chief contribution this volume makes, over and above the particular contributions of each of its remarkable chapters. It is these, of course, which are the most rewarding, and they can be read with fascination and profit quite in-

dependently of one's historical commitments. The editors have done a splendid service in making them available.

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Physical Sciences

Soviet Research in Glass and Ceramics. Chemistry collection No. 2. *Basic Science,* vols. 1 and 2; 497 pp. *Cements, Limes, and Plasters*; 203 pp. *Refractories*; 70 pp. *Glass, Glazes, and Enamels*; 197 pp. *Miscellaneous.* 31 pp. Consultants Bureau, Inc., New York, 1958. $150.

It is not convenient to comment on all of these papers. Rather it is the goal of this review to present some idea of their scientific and technological level—a level which can perhaps be expressed in terms of the experimental techniques, theories, and documentation that are employed.

The papers fall into several categories—theoretical papers, data papers, reviews, and purely engineering papers, plus several book reviews. They have appeared in several journals of applied chemistry and are not from the technological press.

There are numerous papers on phase equilibria, including water systems at room temperature and anhydrous systems at high temperatures. The water systems are carried out with standard techniques and appear to be carefully done. The high-temperature phase equilibria are usually carried out by means of the cone-fusion method; this technique, while rapid and inexpensive, is considered to be no more than exploratory in modern phase equilibria work. As an alternative, cooling curves are used. The method of phase characterization—microscopic and x-ray—is standard. In no case in this selection of papers is there a complete high-temperature phase diagram. Equipment-wise this part of the research reported is inferior.

The 12 papers in the *Refractories* section are carefully done pieces of work, reflecting the interest in high-alumina bodies and in such modern or potential refractories as boron nitride, zirconium carbide, graphite, and the borides of beryllium and magnesium.

In the *Cements, Limes, and Plasters* section, the 28 papers deal with the problems of hydration and with the effect of additives on the whole gamut of cement compositions. The most modern techniques and equipment are used in the studies discussed. Considerable space is devoted to petrographic examination of the various clinkers. The theory of the hardening of Portland cement (and the Russian role in its creation) is reviewed in a long paper. The technical part of this paper is current and quite well presented, but the paper has political and nationalistic overtones, including a quotation from Stalin: "No science can develop and prosper without combat of opinion and without free criticism." This quotation, justifying some of the author's criticism, is countered in the final paragraph of the paper by the following quotation: "Soviet students, armed with laws and methods of Dialectical Materialism, create new, purely scientific concepts, because, without Dialecticalism, there is no science."

Let us pass to another subject. A scientific author has a primary duty of adequate documentation. In these 28 "cement" papers, which cite 3 to 26 references apiece, only two references are to literature of European or American origin. This poses a problem the answer to which one can only guess at: (i) Does this particular series of laboratories lack world literature on cement? or (ii) lack knowledge of foreign languages or of translators? or (iii) reject foreign publications in this field? To this problem one can only report that, for many papers in the "glass" and "basic science" sections of this series of translations, the documentation with respect to world literature is quite adequate. This was especially true in the case of a recently translated symposium on crystal growth (issued by the same publisher), where the citation of world literature was excellent. Perhaps the absence of adequate citation in the "cement" papers as a matter of geographical obstacles.

There appears to be no pattern governing whether or not an author will include citations of obviously pertinent European or American references. In the papers on more strictly engineering and technological subjects, the citations are usually exclusively Russian. One must make allowances for the language barrier and the fact that the lifetime of Soviet science has been sufficiently long to permit the writing of adequate Russian reference books which eliminate the necessity of reference to world literature.

The 31 "glass" papers cover the field of modern glass technology. The techniques and methods of measurement are standard and adequate. The documentation shows wide reading in the world literature. There are several theoretical papers of interest.

One series of four critiques is of interest: (i) a paper criticizing the classical theory of solid phase reaction; (ii) two separate papers criticizing paper No. 1; (iii) an answer by the author of paper No. 1. The classical theory of solid phase reaction from Tamman, Hedvall, and others (metaphysicists, not dialectical materialists, and hence in error) was centered around reaction through solid-solid diffusion alone; the gas phase was not involved except in oxidation-reduction reactions. Critique i says this is in error and that the error is one of philosophical approach on the part of the founders of the theory. This author insists on the importance of vapor phase transfer in solid phase reaction. K. Marx and F. Engels are quoted in support of the philosophy. Critiques ii and iii criticize critique i for the misinterpretation of Marx and Engels. These criticisms are finally answered by the author of paper No. 1. One must point out that in current Western science the contribution of the vapor phase transfer to this reaction is gaining recognition.

In this series of translations the papers with political overtones are rare and are reviews or polemics, never experimental papers.

Finally, one should answer the question, "How many of these papers would pass the editorial board of the American Ceramic Society?" In my opinion, if one ignores the political papers, about 85 percent would be acceptable.

Incidentally, the translations are quite readable. There are occasional errors in transliteration of words, usually Western names. The reproductions are poor but simply reflect the inferiority of the original.

S. ZERFOSS
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On Nuclear Energy. Its potential for peacetime uses. Donald J. Hughes. Harvard University Press, Cambridge, 1957. xi + 263 pp. Illus. $4.75.

Atomic energy is here to stay, and we have to live with it. Therefore we have to know what atomic energy really is, and we have to learn about the fundamental facts which underlie all atomic energy phenomena, from the first horrifying explosions over Hiroshima and Nagasaki to the steadily growing applications for peacetime uses for our day and for the future.

Many books have been written on this subject; this one is distinguished by the ease and the superiority with which the most complicated problems are presented. The expert in the field enjoys reading the clear, well-written text, and the layman who, as a newcomer, looks for a serious, scientifically reliable introduction gets a real treat. Using only two equations in the entire book, the author develops and explains the solid facts of atomic structure, fission, chain reaction, fusion, and the many applications in such a way that he instills in the reader a keen desire to learn and to know still more.

This book mirrors the immense experience that Hughes, a leading scientist at Brookhaven National Laboratory, has had in presenting this subject in lectures to a wide variety of lay groups all over the world, both on this side of the iron curtain and behind it. Therefore his discussions on "The international atom" and on "Safety, security, and the AEC" are stimulating to the reader who is also interested in the impact of atomic energy on the social structure of our time.

The radiation biologist and the health physicist will be gratified to find in a book of this kind, written for the public and for lay groups, a competent discussion of the hazards of atomic radiation, of the genetic effects, and of the radiation safety limits.

A. T. KREBS

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Progress in Nuclear Physics. vol. 6. O. R. Frisch, Ed. Pergamon Press, London, 1957. vii + 285 pp. Illus. $14.

Annual Review of Nuclear Science. vol. 7. J. G. Beckerley, Ed. Annual Reviews, Palo Alto, Calif., 1957. viii + 478 pp. Illus. $7.

In these days, when the *Physical Review* alone prints more than 6000 pages per year, the need for periodical reviews is clearly evident. No one can, without help, keep up with the developments even in his immediate area. The volumes under review are the compilations for 1957 of two series of annual reviews for the nuclear scientist. Both series have been able over the last few years to attract competent reviewers—the obvious prime requisite for success—and both have maintained high standards. Otherwise the two series, though covering much of the same ground, have quite different purposes.

The British compilation seeks to provide not-too-detailed and easily read general representations of the selected topics, with just enough references to the basic papers and current literature to guide the reader in further studies. The most valuable papers are probably those on the atomic masses, by J. Mattauch and F. Everling (A less than 40) and H. E. Duckworth (A more than 40), which provide a thorough and up-to-date discussion of the accuracies of current mass determination, a comparison of data obtained from nuclear reactions and from mass spectroscopy, and tables of the best values. The most enjoyable articles are those by G. N. Walton (on fission physics, with discussion of the fission process, the slowing down of the fragments, the effects on surrounding materials, and the chemical properties of the products) and by R. J. Eden, who gives a short guide to the variety of nuclear models that have been introduced for various purposes. There are two articles on isotope separation, by T. F. Johns (on multistage methods) and by M. L. Smith (on electromagnetic separation). K. F. Smith gives a useful discussion on nuclear moments and spins, with a survey of methods of measurement and an up-to-date table. M. B. Stearns gives a rather dry compilation of work done in the field of mesonic atoms. The book is brought to a fitting close by a short but lively discussion of nonconservation of parity, by O. R. Frisch and T. H. R. Skyrme. Altogether this volume is pleasant to read, useful, and of ephemeral value. The price is high.

The American counterpart, *Annual Review of Nuclear Science,* represents a much more ambitious undertaking: It seeks to provide authoritative and detailed technical coverage of its topics, with complete literature references up to specified dates. The present volume includes three quite outstanding articles. F. Villars gives a searching discussion of the collective model. He brings out very clearly that the simplicity of collective dynamics, as demonstrated by the possibility of describing with high accuracy, and in terms of very few parameters, many data, such as energy levels and moments, is not yet accounted for by the presently available mathematical methods. He emphasizes the inadequacy of the hydrodynamic approximation, but he holds out the hope that the method of "redundant variables" may ultimately achieve the goal of giving a unified description of single-particle and of collective motion.

R. Hofstadter, in his report on "Nuclear and nucleon scattering of high energy electrons," succeeds admirably in his goals—to get across the fundamental ideas of scattering theory and to give an up-to-date report on experimental and theoretical results. M. Gell-Mann and A. H. Rosenfeld give a lucid and authoritative representation of the field of hyperons and heavy mesons and their decay properties. The "strangeness" systematics is fully discussed, as are the consequences of parity nonconservation in weak interactions. Three other physics topics ("Mu-meson physics," by J. Rainwater; "Collisions of $\leq$ 1 Bev particles with nuclei," by S. J. Lindenbaum; "Spins and static moments of radioactive isotopes," by W. A. Nierenberg) are all very competently handled, with emphasis on basic understanding of the physics situation.

The volume contains, further, six articles on chemistry and radiobiology: "Radiochemical separations by ion exchange" (K. A. Kraus and F. Nelson); "Equipment for high level radiochemical processes" (N. B. Garden and E. Nielsen); "Cellular radiobiology" (E. L. Powers); "Biochemical effects of ionizing radiation" (B. E. Holmes); "Vertebrate radiobiology (lethal actions and associated effects)" (V. P. Bond and J. S. Robertson); "Vertebrate radiobiology (the pathology of radiation exposure)" (C. C. Lushbaugh). I, as a physicist, have no competence for judgment, though, because of my interest in nuclear energy, I certainly need information in these fields. In spite of my eagerness to learn, I found that all these articles made very dull reading. On the other hand, a radiobiologist, even with very good understanding of the physics required for his work, will find Villar's highly-rated article completely incomprehensible. Thus, while the physics part of this volume is truly excellent, it remains questionable whether it is wise to collect in one volume review articles, dedicated to the experts, covering such a large diversity of topics.

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Handbook of Magnesium-Organic Compounds. vol. I, *Reactions of Magnesium-Organic Compounds Nos. 1–7284*; vol. II, *Reactions of Magnesium-Organic Compounds Nos. 7285–13395*; vol. III, *Indexes of End-Products of Reactions, Magnesium-Organic Compounds, Authors and Co-Authors.* S. T. Yoffe and A. N. Nesmeyanov. Pergamon, London and New York, 1957. 2048 pp. $72.

This monumental work is divided into three volumes, largely for more convenient handling. The first and the second volumes contain tables, of 13,395 entries, which list the empirical formulas (in the order used in *Chemical Abstracts* indexes) and abbreviated structural formulas of reactants; the Grignard reagents used in the respective reactions; the products formed from the particular reactant and Grignard reagent; and literature references to the bibliography contained in the third volume. Volume III also contains an index of end-products arranged in order of empirical formulas, as well as an index of RMgX compounds, excluding the simplest.

There is a foreword by W. Wardlaw. An English translation of the preface and of the introduction, by A. L.Mackay, is admirably suited for helping one make the fullest, most effective use of the volumes.

The preface of the book emphasizes, to an unusual extent, the contributions of Russian chemists to the development

of the Grignard reagent. The Russian chemists have done very significant work in this area, but it is regrettable that no mention is made of workers such as Kohler, McKenzie, Oddo, Ruzicka, and Tiffeneau. For example, Kohler's classical studies not only developed, at an early stage, some fundamental aspects of organomagnesium chemistry but also provided an uncommon and extensive contribution to our knowledge of conjugated systems.

A cursory examination of some of the entries reveals the incorrect spelling of the names of some authors. This is not an overly serious matter when one reflects on the equally inadvertent but much more frequent misspellings or transliterations that are made in the names of Russian chemists by non-Russians. However, the same cursory examination did reveal the omission of some literature references concerned with the broad development of the Grignard reagent and its reactions.

In a sense, this compilation supplements the more critical and readable *Grignard Reactions of Nonmetallic Substances,* by Kharasch and Reinmuth. That one-volume work is more restrictive in its scope (as its name indicates), but it covers two more years of the literature (up to *Chemical Abstracts* of June 1950).

The authors have done a great and splendid service to organic chemistry, and this *Handbook* should be available in libraries, where one can confidently count on its extensive use as a valuable work of reference.

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Dynamic Programming. Richard Bellman. Princeton University Press, Princeton, N.J., 1957. xxv + 342 pp. $6.75.

The book under review may be considered to be a mathematics book, since the author is a well-known mathematician. However, it is not a book primarily about mathematics, as all too many mathematics books are apt to be. It is devoted to developing mathematics in response to problems arising in the social, business, military, economic, and political worlds as well as in engineering and the natural sciences.

Here, then, one may find direct statements of the applications of the mathematical theories developed, together with the construction of theories to solve specific classes of problems, such as inventory problems, depletion problems, and scheduling problems in general. The title *Dynamic Programming* refers to development of a dynamic optimal policy or program as a guide for the making of time-dependent decisions in complex problems involving many variables. Optimization may refer to maximizing net profit, to minimizing risk probabilities, to minimizing storage space, to minimizing delivery times, and so on.

Dynamic Programming takes its place among the comparatively recent attempts to develop mathematics to meet problems of modern civilization and was undertaken in somewhat the same spirit as were John Von Neumann's study of the theory of games and Abraham Wald's of the theory of sequential analysis.

While the book includes many problems indicating the scope of applications, it is not a book that can be easily read for its philosophical content alone, since the author uses concepts of advanced mathematics with ease and makes comparisons which require mathematical experience on the part of the reader.

The need for some serious attention to higher-dimensional geometry and analysis in the undergraduate curriculum is again seen in this book, which could be read with profit by leaders in a wide variety of fields if they had the capacity to assimilate its contents.

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Department of Mathematics,
University of Wisconsin

Elementary Theory of Angular Momentum. M. E. Rose. Wiley, New York; Chapman & Hall, London, 1957. x + 248 pp. $10.

The student of quantum mechanics is soon introduced to the simplest properties of angular momenta, whereupon he is often inclined to believe that his knowledge of the subject is complete. In later stages of his development he will meet with increasing frequency references to sophisticated general theorems, usually accompanied by the casual remark that they follow from group theory, and his knowledge has not advanced by more than the realization that there is evidently more to the matter than he thought.

At this point he is well advised to turn to the new book of Rose, which neither minimizes the complexity nor introduces an unnecessarily elaborate mathematical apparatus. Thus, the orbital angular momentum of a single particle is used as an illustration and not as a substitute for the general definition which requires the consideration of rotation; on the other hand, there is wise economy insofar as the theory is directly based upon the truly necessary geometrical properties of infinitesimal rotation rather than upon the actual but immaterial fact that one deals with a special case of continuous groups. A logical development leads from this start to the coupling of two and three angular momenta, the Wiger-Eckert theorem, Racah coefficients, and other more complex aspects of the general theory treated in part A.

It cannot be expected that the relatively short second part, part B, would deal with more than a fraction of the many applications. In particular, it stresses those relating to angular correlations and nuclear reactions, and it contains an introduction to the properties of static moments. Nevertheless, the reader will be equipped to acquaint himself more thoroughly with some of the special literature, quoted as reference.

F. BLOCH
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The Inner Metagalaxy. Harlow Shapley. Oxford University Press, London; Yale University Press, New Haven, 1957. xiii + 204 pp. Illus. + plates. $6.75.

This book, by the former director of the Harvard College Observatory, has been awaited with keen interest for several years by all workers in the field of extragalactic astronomy. It covers the region of the extragalactic universe within reach of telescopes of moderate size; this region Shapley calls the "inner" metagalaxy, following a nomenclature introduced some thirty years ago by K. Lundmark of Sweden. The exploration of this domain has been for several decades the special interest of the Harvard College Observatory, and it is essentially a synopsis of this work, carried out by Shapley and his coworkers, that the reader will find described in this volume, on a semipopular level.

After an introduction designed to assist the nonspecialist, Shapley describes the various surveys or censuses of faint galaxies in several strategic locations of the sky and the main conclusions derived from them relative either to the large-scale distribution of galaxies—the evidence for density gradients, for clustering, and so on—or to the distribution of the absorbing material—the cosmic "smog"—in our own Galaxy. This section comprises seven of the 14 chapters of the book and may be the most attractive and useful part, for it gives a clear and well-organized summary of results that, until now, had remained scattered over many publications. The details of the galaxy counts are conveniently summarized in five appendices. There is also a brief discussion (one chapter) of the thousand brightest galaxies in our imme-

diate neighborhood, as listed in the classical Shapley-Ames *Catalogue* of 1932. One chapter, reprinted from an article published by Shapley in the *American Scientist* in 1956, reviews rather briefly the Harvard studies of the Clouds of Magellan.

Two chapters dealing with the diameters of the Galaxy and the distance to its center are, perhaps, not very relevant to the subject matter but are of more general interest and in line with the purpose of the book: to present a well-rounded account of all Shapley's main "galactic and extragalactic" studies. The final chapter deals with miscellaneous subjects related to the orientation and evolution of galaxies and tests for possible intergalactic absorption.

The line illustrations are adequate, but the plates are disappointing. This, however, is a rather minor defect, since this is not a "picture book." All students of the extragalactic field will be grateful to Shapley for this very valuable compendium of three decades of "trail blazing work," carried out under his direction at the Harvard Observatory.

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Russian-English Glossary of Nuclear Physics and Engineering. Russian-English Glossary of Solid State Physics. Russian-English Glossary of Electronics and Physics. Consultants Bureau, New York, 1957–58. 195 pp.; 90 pp.; 343 pp. $10 each (series rate available).

These volumes represent a first step in the development of a complete Russian physics dictionary. A number of other specialized glossaries are still to be issued; ultimate publication of a single revised dictionary is envisaged. The present issues, then, are to be judged in the light of their rather transient character.

Within their defined scope, the glossaries appear to be first class. The compilers have avoided the common mistake of presuming that there is a one-to-one correspondence between Russian terms and English equivalents; often two, and occasionally three or more English terms are listed. The translations are realistic, and current terminology is used effectively.

Nevertheless, I was somewhat shaken by the enormous number of terms. These three volumes furnish more than 30,000; the completed text will probably have 100,000. That would make a rather large book to thumb through. Is such a large number necessary? My opinion is negative on this point. For example, *edinitsa* means "unit," and is so listed. It then is followed by ten other phrases (such as length unit, mass unit, energy unit) each one of which is a literal translation of a pair of Russian words (*edinitsa dliny, edinitsa massy, edinitsa energii*, and so on). This practice adds nothing but length to the volume. One should take it as a fundamental rule that no phrase should be inserted in the glossary whose precise meaning can be found by a literal translation of its individual members.

One of the outstanding features of the *Electronics* glossary is a listing of American equivalents of Soviet vacuum-tube designations. This inclusion and that of a series of Russian abbreviations are exceedingly valuable features.

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Radiological Physics. M. E. J. Young. Academic Press, New York, 1957. x + 365 pp. Illus. $7.50.

This book is intended for the use of candidates for diplomas in radiology from the Royal College of Physicians of London or the Royal College of Surgeons of England or of candidates for membership in the Society of Radiographers. The text is thus designed around the requirements for these examinations, but it should, nevertheless, be of value to the many workers who want a ready reference or who wish to review the status of medical physics.

The text is a well-written basic review of the field. Mathematical treatments of the subject matter are avoided for the most part. A useful feature is the group of examination questions at the end of each chapter.

Two introductory chapters review the fundamentals of nuclear physics, thermionic emission, vacuum tubes, and electronic circuits. The production of x-rays is reviewed in two chapters, which separate the subject into the energy region above and that below 400 kev. Chapters on the naturally occurring radioactive substances and the interaction of radiations then set the stage for subsequent chapters on diagnostic radiography, the chemical and biological effects of ionizing radiations, the therapeutic use of gamma radiations, and the production and medical use of artificial isotopes. The text concludes with a chapter on health hazards and radiological protection.

The volume should be well received by those who wish a nonmathematical introduction to the subject of radiological physics.

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Social Sciences

Pleistocene Man at San Diego. George F. Carter. Johns Hopkins Press, Baltimore, Md., 1957. xvi + 400 pp. Illus. $8.

The dogmas that "early man" in the Americas dates back 10,000 to 15,000 years and that the American culture is an isolated tradition have been giving ground. The thesis of this provocative book is that man has lived in the San Diego region of California for some 30,000 years and perhaps 80,000 years and that he witnessed the changes in climate and sea-level of the Wisconsin glaciation. Evidence is developed from studies of geologic, geomorphic, and climatic phenomena and from 17 archeologic site areas, which are then placed within the suggested geochronologic framework.

Among the basic concepts considered are the eustatic theory of sea-level changes; stability of the area, particularly during the Upper Pleistocene; a time-sequence of soils and soil profiles; and correlation of the 25-foot subaerial terrace with the last interglacial of the Wisconsin, on the theory that there has been no sea stand higher since that time. The thesis is supported by data from other areas of California, from other parts of North America, and from other countries, and there is a variety of evidence.

The archeologic sequence is developed on the basis of early work of M. J. Rogers, one of the first systematic investigators in the area. Carter has reinterpreted the archeology on the basis of recent field work and of his knowledge of the region and adjacent areas; he has defined an older tradition and has greatly expanded the time scale of the occupation. Many details are presented, but archeologists generally will not be satisfied with the methodology. Detailed site reports and further analysis of the implements are still needed—a situation of which Carter is fully aware. It is regrettable that reports and artifacts from previous work were not accessible, so that details and comparative data on earlier finds could have been presented.

Stone-working techniques, as indicated by the artifacts and tool-manufacturing debris from the older horizons, are considered, and the need for more research is noted. Many archeologists have reserved judgment or have questioned the view that the workmanship is that of man, but the presence of associated hearths, charcoal, fire-broken rock, burned bone, and marine shells supports the view that the presence of the material in the valley fill and elsewhere is more than fortuitous.

The book is well illustrated with photographs, maps, charts, diagrams, and

line drawings. This is a multidisciplinary approach which, when refined and applied to other areas, should result in further progress in the study of climate change, geomorphology, archeology, and geochronology.

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The Economics of Under-Developed Countries. Peter T. Bauer and Basil S. Yamey. University of Chicago Press, Chicago; Nisbet and Cambridge University Press, London, 1957. xiii + 271 pp. $2.25.

This volume on the economics of underdeveloped countries is the most satisfactory introduction to the subject yet to appear. In 257 pages of lucid exposition the authors, with exceptional understanding and insight, review the major economic influences governing the conditions of life in low-income countries.

The treatment is presented in two parts. Part I, a descriptive and analytical section of ten chapters, is designed "to illumine certain features of the economic landscape of the underdeveloped world." It does more, for in chapters on natural resources, human resources, and capital, it discusses many of the basic requirements for economic growth and improvement. Part II deals with the place of government in promoting economic development. Here in five chapters are reviewed the role and special functions of government and some of the major policy issues relative to the encouragement of capital formation, agriculture, and manufacturing industry.

Because of the normative aspect of the policy conclusions the authors are explicit in stating their political and social criteria. They favor economic development based on diffused decision-making in private enterprise, encouraged by public policies that widen the range of alternatives open to producers and consumers. This rationale for limited government participation is well stated. It is a statement that will be informative to many of those who accept the philosophy as well as to those who do not.

The authors have treated the subjects covered so well, that economists and lay readers alike will wish that more consideration might have been given to several neglected aspects of the subject. The relationship between economic improvement and population growth deserves a fuller treatment than it receives. In view of the recognized importance to economic development of skills and education (page 129), not to mention science and technology, it is unfortunate that the role of these influences is not elaborated and that the policy requirements are not developed. For this the authors may be excused in view of the inadequate state of knowledge about the matter. But even a little illumination would have made this dark corner more understandable.

Another neglected and difficult subject is what may be called the strategy of economic development. On what basis can men determine which things must be deliberately undertaken first and which later, by government or by private citizens, and which things can be left to follow in due course? The criteria for judging priorities have not yet been formulated, as those engaged in economic development well know. Yet, one senses that these authors would have something worthwhile to say on the subject, and one wishes that they had recorded it here.

But these reservations merely reflect the appetite for more. Within the range of subjects chosen, the authors have made an important contribution to a better understanding of economic development in the underdeveloped areas of the world. As stated on the flyleaf, the book is in fact "for the general student of affairs as well as for the economist."

ROBERT D. CALKINS
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Year of Crisis. Communist propaganda activities in 1956. Evron M. Kirkpatrick, Ed. Macmillan, New York, 1957. xix + 414 pp. Illus. $5.50.

As a sequel to the author's *Target: The World*, published in 1956, this study provides an extensive, if not exhaustive, analysis of near-current propaganda activities of communism. For clarity and refinement, it draws upon the earlier work as a point of departure. The analysis of the subject pivots upon the theory of Soviet propaganda as well as Soviet policy in relation to developments specifically ensuing from the 20th Party Congress. The organization and direction of Communist propaganda and the major world-wide themes stressed in 1956 are treated prior to a discussion of events within specific target areas and of operating media and channels of communication. Evron Kirkpatrick emphasizes the major situations which conspicuously shaped the course of Soviet propaganda activity in the period: (i) the redefinition of tactics and approach of world communism in Moscow; (ii) the de-Stalinization campaign; (iii) events in Poland and Hungary; (iv) the Near East crisis; and (v) the expanded role of Communist China as a source of propaganda and doctrine.

The editor apparently has attempted to produce, in this sequel, a book which will be nearly beyond criticism from the standpoint of organization, method, and content; and as far as I am concerned he has been demonstrably successful. Some exceptions, perhaps, should be noted. It is not clear, for example, why the word *socialism* is persistently set in quotation marks. The intent presumably is that of showing contrast with non-Soviet socialism, and thus an error in fundamentals results, since the so-called separate concepts are identical with respect to economic philosophy and nearly identical with respect to political philosophy; neither permits full expression of both political and economic democracy in the normative sense. This elementary confusion lessens, to some extent, the usefulness of the book as an instrument of political and psychological warfare.

In his discussion of the Moscow-Peking axis in propaganda activity with respect to increasing operations in the Asian target area, the reasoning of the editor is less than clear. The major struggle in southern Asia today is for the minds of the "overseas Chinese," since, for Peking, these groups provide built-in cadres for subversion. The gradual enhancement in prestige of the Free Chinese and the Government of the Republic of China—and their strong political warfare activity among the overseas Chinese—presents a vital challenge to the governing group on the mainland. The journeys of Chou En-lai in Southeast Asia during 1956 were essentially, although not wholly, for the purpose of engagement with the overseas Chinese. The editor, having failed to elucidate these facts of life in Asian propaganda activity, will elicit no especial laurels for his presentation of the case in point.

Some inaccuracies in Table 2 (page 82) should be corrected in any second printing. For example, depending on the editor's definition of a unicameral legislature, that of Laos is instead bicameral in a restricted sense rather than unicameral, with an upper-house *Conseil du Roi*; so, too, is that of Cambodia, with its *Conseil du Royaume*. The National Assembly (lower house) of each country is composed of 60 and 93 members, respectively, rather than of 39 and 257, as the table indicates. Several other errors may be noted, such as the simple mistake of stating that the Senate of the Philippines comprises 102 members and the House of Representatives, 24, instead of the reverse. Unfortunately, the incorrectness of this table is so marked that its usefulness is negligible, and this reflects on numerous other tables throughout the book.

Quite aside from such minor points, however, on balance Kirkpatrick has published a noteworthy volume, reveal-

ing in very great detail indeed the predatory efforts of Moscow in this field. Its study should be required of all concerned with the exigencies of world affairs and their relationship to American national policy. Kirkpatrick could make an even greater contribution in this area, of course, should he have the courage to trace through aspects of Communist propaganda in the major nations of the West, expanding upon his chapter X and including the United States and Canada.

DONALD P. RAY

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Principles of Perception. S. Howard Bartley. Harper, New York, 1958. xii + 482 pp. $6.50.

In a nicely balanced arrangement, the 21 chapters of this textbook fall into three general areas of consideration. The first five deal with the definition, nature, and development of perception. The next 12 take up the separate senses and the experimental studies that reveal the human reactions to environmental and physiological impingements. The remaining four chapters bring the study of perception alive by deviating from the traditions of experimental psychology in presenting, in a single book, the studies in social perception, perceptual anomalies, and the everyday experiences in which human behavior reflects more than mere response to stimuli.

The student—the designated reader of this book—is introduced early to the changing appreciation among psychologists of the nature of perception. The old distinction between sensation and perception has dissolved, and the recognition of the whole body and past experiences is emphasized.

The traditional experimental chapters are fairly straightforward. Although the author's interpretation pervades the discussion, it is not pushed, and those of others are presented. This is particularly true of the chapters on vision, which are the best.

The two chapters on hearing cover many of the traditional experiments, but the conclusions that audition is greatly divorced from the domain of space and, because of this, plays the prime role in dealing with abstract ideas may come into some debate. And the reader may wonder what "fusion of intermittent white mice" (page 295) has to do with auditory flutter fusion of interrupted noise. There are not many such typographical errors.

As every reader of science fiction and the newspapers knows, the understanding of the behavior of man in an environment of unpredictable complexities is definitely in demand, and psychology, with its stimulus-response analysis, has not yet supplied this. Bartley's book is a good step in the right direction. Man is an integrated organism, developed and conditioned through an existence of successful and unsuccessful past experiences, and the kind of treatment provided by this text is what is needed for the interpretation of man's responses. Although somewhat bounded by tradition in that experimental results are presented in terms of the average observer, when what is eventually needed is an explanation of why individuals respond differently, the book is a contribution to psychological theory and an excellent step forward in both presentation of subject matter and in ideas. It should make a thought-provoking text, and the first and last areas of consideration should appeal to everyone.

MERLE LAWRENCE

Physiological Acoustics Laboratory, University of Michigan

Traité de Paléontologie. vol. VII, *Primates: paléontologie humaine.* Jean Piveteau. Masson, Paris, 1957. 675 pp. Illus. + plates. F. 12,800.

The latest of the seven major divisions of the *Traité de Paléontologie* is an excellent and comprehensive reference manual and a useful textbook on the paleontology of the primates. It is an authoritative review of the subject and covers the order in its diversity from the primitive tree-shrews to man, and in geologic time from certain minute, middle Paleocene forms of the Rocky Mountain region to the subrecent anthropoids of the Old World. Essentially, the book is written from the point of view of a paleontologist and deals with the materials available to the paleontologist for study.

The text is divided into two parts, the first being the general paleontological history of the primates and the second and larger section being devoted to human paleontology. The more important and classical discoveries and occurrences are treated historically, and stratigraphic details are given where such information is pertinent. The text is profusely illustrated, and in most instances the original plates and figures used by the various investigators have been reproduced, with excellent results.

Fortunately, from the standpoint of greatest utility, Piveteau has given more attention to morphological detail and comparative study of the materials and rather less to taxonomy and problems of classification. While rather generally following accepted interpretation in relationships, he has presented the pros and cons where important differences of opinion exist. He has, for example, followed Le Gros Clark, G. G. Simpson, and others in including the tupaioids in the primate order; on the other hand he has excluded the early Tertiary apatemyids, agreeing with Jepsen, Hürzeler, and certain others. The problematical Pliocene anthropoid *Oreopithecus,* however, is treated as a hominid, a point of view vigorously maintained by Hürzeler but not too generally held by others.

Such criticism of the treatise as I may feel justified in offering, other than comments on certain details of classification, should properly be restricted to the area of my own studies, the earlier North American assemblages. While I find that consideration of the earlier Old World materials, particularly from the European Eocene, appears well developed, the North American picture is, for perhaps justifiable reasons, less adequately covered. Except for the phenacolemurids, little mention is made of the diversified and geologically oldest primates known, as recorded principally by J. W. Gidley and G. G. Simpson, from the middle Paleocene of the Rocky Mountain region. Moreover, the upper Eocene primates of North America, particularly those described by Chester Stock from the West Coast region, have been completely overlooked. Nevertheless, the American paleontologist who possesses a first-hand acquaintance with the New World faunas will find the treatise exceedingly useful in its better coverage of the more remote and, to him, less accessible materials. I think it should be noted, and with embarrassment, that there appears to be no up-to-date paleontological treatise or textbook on primates in the English language which compares with it.

C. LEWIS GAZIN

Smithsonian Institution

Explorations in Role Analysis: Studies of the School Superintendency Role. Neal Gross, Ward S. Mason, and Alexander W. McEachern. Wiley, New York, 1958. 379 pp. Illus. $8.75.

This is a well-written report on a major research study. The contribution the book makes to our knowledge divides into the two major areas that are clearly reflected in its dual title. On one hand, the authors give us some insight into important characteristics of the position of the public school superintendent. On the other hand, a pivotal concept in social science—namely, that of role—is tested for its power to make meaningful certain empirical data collected from superintendents and school board members.

The research study concerns a sample

of 105 public school superintendents in the state of Massachusetts and 508 school board members to whom these superintendents are responsible. Data for the study of these superintendents' roles were collected through carefully structured interviews, lasting a full day for each superintendent and averaging two hours for each school board member. The interview with the superintendent covered such topics as the division of labor between the superintendent and his board, his satisfaction with his job, his evaluation of individual board members, his freedom of action, and his perceptions concerning the persons in the community who influenced his decisions. Many of the same areas were covered, but less extensively, in the interviews with the school board members.

The analysis of the data collected in these interviews centered about role consensus, or the degree of agreement among individuals regarding a characteristic of the superintendent's position. A number of hypotheses were formulated and tested against the empirical data. These hypotheses concerned a wide range of variables presumed to be relevant to the subject of the extent of agreement among superintendents. Many of the hypotheses were given support by the data.

Despite the fact that the authors treat a large mass of empirical data, the emphasis of the book is clearly theoretical. In this regard, the authors first provide thorough but succinct discussions of their key concepts and then strike out in new directions. In so doing they extend the usefulness of the concepts of role analysis for social scientists.

Although the reader who approaches the book with a desire to learn more about the public school superintendent's job will not be disappointed, it is the advanced student in the social sciences who will find it most rewarding.

JOHN K. HEMPHILL
Educational Testing Service, Princeton, New Jersey

Method and Theory in American Archaeology. Gordon R. Willey and Philip Phillips. The University of Chicago Press, Chicago, 1958. ix + 270 pp. $4.75.

The basic material in this book first appeared in two articles in the *American Anthropologist,* in 1953 and 1955. The flood of comments and criticism which followed so stimulated the authors that they rewrote and combined the original papers, added an introduction in the form of a brief article previously printed in another journal, and produced the present volume. The chief objective of the Willey and Phillips studies was to determine if it would be possible to make a gross classification of all New World archaeological data under a small number of broad developmental stages, and if the results would be useful. To this end they reviewed and evaluated previous concepts and practices and reached the conclusion that, so far as the New World is concerned, the theoretical aspects of archaeology are definitely anthropological and that the archaeologists can not escape giving consideration to some of the basic questions of anthropological theory. Thus, the big problem for the archaeologist is the interpretation of his findings as social and cultural phenomena in the anthropological sense.

The authors believe that archaeological data must be organized in units that correspond to the social and cultural aspects of anthropology. They propose and define a series of such units and, after extended discussion, indicate that it is only in those of the smallest magnitude that there are clear-cut social equivalents, although there are vague hints of such in some larger units. Culturally, the situation is better.

The remainder of the book concerns the historical-developmental approach toward a synthesis of New World prehistory. The archaeological data are grouped in five major stages, and the manifestations in the various areas belonging to each stage are discussed. The categories to which certain types of remains are assigned probably will be a matter of some disagreement, but an interesting review of aboriginal culture-history in the New World is presented. The book is well written and provocative. It is for the advanced student and professional anthropologist rather than the general reader.

FRANK H. H. ROBERTS, JR.
Smithsonian Institution

Principles of Economic Policy. Kenneth E. Boulding. Prentice-Hall, Englewood Cliffs, N.J., 1958. viii + 440 pp. $7.95.

This is a book with a variety of ingredients: rigorous economic reasoning, ethical goal-setting, wise and at times humorous *aperçus*—and all bound together in a clear and unpretentious language. The architecture of the book is very simple. It uses as foundation a discussion of four policy objectives: economic progress, economic stability, economic justice, and economic freedom. On this basis are erected as columns the various types of economic policy—namely, fiscal and financial policies, social ("income maintenance") policy, foreign economic policy, and government policies concerning business enterprises, agriculture, and labor. These columns are joined in a dome formed by a "science" of government eventually to replace the present "craft" of government. This science grows out of a theory of value systems. The book is stimulating, honest (it emphasizes not only what we know but also what we do not know), and tolerant (it avoids black-white statements and eschews dogmatism).

In a book dealing with a topic as broad as economic policy it would be a miracle if the reviewer did not find that important aspects are not adequately treated. Economic policy is discussed as if it is made in a social vacuum. Economic policy is determined not only by the decisions of philosopher-statesmen but by political processes and pressures which embrace both group and individual self-interests and responsiveness to ideals. Even a book on principles should convey to the reader a notion of the social and political forces which move economic policy. Our problems in economic policy cannot be understood without being seen in political-sociological perspective.

Scattered throughout the book are a number of cogent remarks about deficiencies and gaps in a private market system which make government economic policy a necessity. A full chapter dealing with this topic in a systematic manner would, I feel, have been useful. A theory of government economic policy should, in my opinion, be based in part on the theory of the deficiencies in the pure market economy.

There is no discussion of economic policy as it relates to such important fields as promotion of transportation and conservation and development of natural resources and stimulation of research. Nor is there more than passing reference to education and training, which are government responsibilities of great economic importance.

It is obvious that no book of manageable size can deal with all aspects of economic policy. I mention these omissions not so much by way of criticism as in order to indicate what the reader can and cannot expect to find.

I would not have the right to call myself a brother-economist if I did not find some statements in the book to which I take exception. An example is the rather conventional treatment of tax incidence, particularly of the corporate tax, with which I cannot agree.

I believe this work will be a great success, not only because of its rewarding substance but also because of the novel practice of summarizing each chapter in doggerel, all of which is good and some of which is superb in humor and wisdom. Therefore, my advice is:

> If you are bored with political economy
> Try Boulding's economic poetry.

GERHARD COLM
National Planning Association, Washington, D.C.

Changing Values in College. An exploratory study of the impact of college teaching. Philip E. Jacob. Harper, New York, 1957. 174 pp. $3.50.

This book grew out of attempts to plan research on the changes in attitudes or "values" of students which result from taking social science courses. The author has discovered a varied array of data on the broad question reflected in the title. He provides an extremely useful bibliography on such topics as general and special surveys of values of college students, studies of values of college graduates, studies of changes in values and attitudes while in college, and before-after studies on the effects of particular teachers or courses or methods of teaching.

The author also chooses to view the current situation with alarm. On the basis of a selection of data from studies of varying quality we are given a "profile" of American college students. Among other things, these students are "remarkably homogeneous" in values, "gloriously contented" about the present and future; "unabashedly self-centered," they are dutifully responsive towards government but don't initiate service in a clear loud voice; they set great stock by college mainly for purposes of vocational advancement. College education does not liberalize the student as much as it socializes him for a place in the ranks of American college alumni. He is more opposed to "welfare economics" than are most citizens. Students are more alike in attitude as seniors than they were as freshmen. Massive changes of student values do not result from concentrating in social science, from taking particular courses, or from taking courses with particular instructors. A few institutions appear to have the ability to really develop student values, and the author tries to tell us why.

Such a summary raises troublesome questions regarding the basic aims and achievements of American education. We think that we do much more than merely beget the social kind, from generation to generation, in such a way that the niches and roles of our society are somewhat adequately filled. This book presents us with a flat challenge on this score and requests that we clarify what the "more" is that we are after. This is a good service.

Validity of the data as presented is at best a moot point. Jacob rests much of his argument on proportions of answers to questionnaires, which are notoriously prone to change with even minor rewording of the specific questions used. Further, some behavioral scientists would assume, to begin with, that basic values are set down firmly in students long before the college years and would see Jacob's alarm as due to naivete with regard to "values" and how they grow. Others would argue that the failure of social science courses to change students' values is merely a sign of the maturity of social science in its move from a normative to a descriptive base.

The book ends with some suggestions for further research, which lack the boldness of the book's main challenge. I would prefer to see a sharper use of present research tools, a more refined analysis of some of the present data, a further development of theory and measurement of values, and then a head-on research analysis of the impact and outcomes of American education. May this book stimulate the production of better ones.

WILLIAM W. LAMBERT
Cornell University

The Administrative State. An introduction to bureaucracy. Fritz Morstein Marx. University of Chicago Press, Chicago, 1957. x + 202 pp. $4.

This is a book about bureaucracy. It has been only six years since "the bureaucracy" was made to play the villain in one of the most popular yet tragic farces of the decade—the McCarthy hearings. To many students of government these hearings will always mark the time we allowed ourselves the expensive indulgence of holding the notion that bureaucracy is an inherent evil in all governments; more particularly, that it personifies the evil behind things we do not agree with. What capital the misguided Senator could have made out of a man with the name Marx who denies this premise!

To Morstein Marx, eminent student, respected teacher, and proven practitioner of bureaucracy, the bureaucracy is a powerful resource, of the order of gun powder, uranium, or ideology. It can be managed either wisely or unwisely and is, accordingly, either a benefit or a malignancy. Our problem is that we have more to learn about managing it. "First we must ask whether technological developments will significantly modify . . . the structure and workings of [bureaucracy]. Will machine-controlled rationality . . . supersede day-by-day supervisory authority? Is the citizen being choked by merciless 'processes'? . . . we should give some thought to the possibility of coming to a more satisfactory interlocking of political responsibility and administrative responsibility. . . ." And, finally, he says that "the basic effectiveness of the constitutional state is linked today in constantly increasing scope with both the productivity and the integrity of the administrative system—and perhaps these two are essentially the same. It is in this vast realm that modern government performs the host of activities which in their detail hardly meet the eye but which extend in a thousand ways throughout the economic and social order." From his intimate knowledge of the Prussian, French, Swiss, British, and American bureaucracies, Marx presents a scholarly diagnosis embodying a prescription appropriate to the treatment of the national fever for which the unfortunate Mr. McCarthy has now become the symbol.

WILLIAM J. GORE
Graduate School of Business and Public Administration, Cornell University

The Teaching of Hygiene and Public Health in Europe. A review of trends in undergraduate and postgraduate education in nineteen countries. F. Grundy and J. M. Mackintosh. WHO Monograph Series No. 34. World Health Organization, Geneva, 1957 (order from Columbia University Press, New York), 254 pp. $5.

The phenomenal advances in the basic medical sciences during the past seventy-five years have made it necessary to undertake sweeping reappraisals of the teaching of medicine. This monograph reports trends in thinking and in the practice of teaching one aspect of medicine in the schools of 19 European countries. For some time it has been recognized that progress in the field of health was dependent to a large extent on the level of the studies for the preparation of both specialized health administrators and medical practitioners. In the period following World War I, schools of hygiene and national institutes of hygiene were created in many European centers. In the main, these institutions have fulfilled their functions with a fair degree of success. In recent years there has been an increasing dissatisfaction with the training given to medical students in the field of preventive and social medicine. It has become clear that the protection of health exclusively from the hygienic and sanitary points of view, without reference to social and economic conditions, cannot be fully effective.

In this report brief accounts of the facilities available and the teaching methods used in the individual countries are recorded, but the major portion of the monograph is devoted to a review of the history and philosophy that underlie the teaching of preventive medicine and public health.

The authors of this review possess high technical qualifications and long experience in the teaching and practice of public health in Great Britain. Their report is well documented and presents many

interesting details on teaching methods. More important, it opens new perspectives not only on training but also on public health practice.

Though this study is limited in scope to the European scene, it should be read by all concerned with the training of undergraduate and postgraduate students in the health field in other parts of the world.

An excellent introduction to this volume is provided by Jacques Parisot, distinguished pioneer in social medicine at the University of Nancy.

HUGH H. SMITH

Tucson, Arizona

The Eye Goddess. O. G. S. Crawford. Macmillan, New York, 1957. 168 pp. Illus. $10.

The wanderings of a pair of bulging eyes, capped by arching brows that meet over the nose, is the subject of *The Eye Goddess*, by the late O. G. S. Crawford, who, at the time of his death in November 1957, was editor of the distinguished archeological journal *Antiquity*. First identified by Max Mallowan in his excavations of the Eye Temple at Brak, Syria, the eyes are found on many figurines made around 3000 B.C., during the Jemdet Nasr period, before the pyramids had been built.

Mallowan identified the eyes as the most outstanding feature of the portrait statuettes of the goddess Inanna, alias Ishtar and, later, Aphrodite. She begins as a Neolithic goddess of fertility. Her essential symbol, the eyes and brows, was carried westward through the Mediterranean to the Atlantic, north to the British Isles, and south to the Canaries. It also migrated southwestward into Ethiopia.

When his editorial duties permitted, Crawford traced the wanderings of his favorite orbs, both by personal travel and in the literature. The product is a model study of the oft-challenged principle of diffusion, as sound as it is entertaining. Readers expecting a new Heliolithic cult or Lost Land of Mu are warned to stay away.

The cult of the popping eye has survived in Syria until today in the form of eye-charms hung inside the cabs of trucks and taxis, obscuring vision while giving assurance of safety to the drivers. They are comparable to the pairs of baby shoes dangling in front of many American windshields.

With the eyes traveled other designs, such as axes and daggers and the figures of obscenely fat women. None of these, however, follows as consistent an itinerary as does the ocular motif. As this moved outward from its source during two or more millenia, the artistic renderings of the subject became decreasingly realistic until, in Ireland, the eye motif was thrown completely out of focus. The end product was a set of symbols resembling a field of croquet wickets after an earthquake.

What Crawford's pet subject has done for archeology is to remove discussion of the diffusion-of-art motifs from the realm of speculation and set it up in the grassy fields of science. He has further shown that a motif can move farther afield than its original interpretation or function. His study is also of use in the mapping of the sea routes of the Mediterranean and Atlantic during the Neolithic and Bronze ages. The book is written in a lively style and is beautifully, even extravagantly (for these days), illustrated.

CARLETON S. COON

University Museum,
University of Pennsylvania

An Introduction to Method in Psychology. W. M. O'Neil. Melbourne University Press, Melbourne, Australia, 1957 (order from Cambridge University Press, New York). ix + 155 pp. $4.50.

O'Neil, McCaughey professor of psychology at the University of Sidney, has written a lucid introduction to the method and logic of science as it applies to psychology. To start with he assumes, first, ". . . that things exist independently of our knowledge of them, . . . second . . . that there can be no duality of the rational and empirical, of the universal and the particular, . . . and . . . third . . . that logic is an empirical study whose principles are determined by things themselves." The tone is, however, positivistic, and O'Neil acknowledges a debt to discussions with Professor Herbert Feigl. The second assumption leads him into views which he recognizes are somewhat idiosyncratic, and he carefully calls the reader's attention to the contemporary dominance of other approaches. All major illustrations are from problems of psychology, including the explanation of the moon illusion, factor analysis, duplicity of visual receptors, inheritance of intelligence, and so on. Case study and survey methods are discussed, although the emphasis is upon experimental methods.

The approach is quite elementary, starting with syllogistic reasoning and immediate inference and going on to scaling, measurement, and functional relationships. O'Neil has a gift of saying simply and clearly what he means—as an example, on the ultimate weakness of refined methods of introspection, he states, "Now all that it seems possible to make of this is that observers can [illegible] almost anything they want if they [illegible] hard enough or are worked on hard enough—a point that is always worth remembering." As another example he points out how *a posteriori* alteration of assumptions can make scientific inquiry degenerate into "hypothesis-saving instead of hypothesis-testing."

Generally speaking, I have only two quarrels with the book: first, a syllogistic approach in scientific reasoning, although not outmoded, tends to lead to the postulation of elements and the like, a device of limited value in psychology—more emphasis should be given relationship, function, and dependence; and second, the view that a formal (syllogistic) proposition is a metaphor, while correct in the sense that all models represent analogies, can lead only to chaos if the metaphor notion intrudes into the logical manipulations. The book is an interesting, pleasant introduction to the logic of psychology, which should make clear to readers from other scientific fields that psychologists use essentially the same approach found in the other empirical sciences.

DAVID A. GRANT

Department of Psychology,
University of Wisconsin

New Books

Malaya: A Political and Economic Appraisal. Lennox A. Mills. University of Minnesota Press, Minneapolis, 1958. 245 pp. $4.75.

The Grassland and Fodder Resources of India. R. O. Whyte. Indian Council of Agricultural Research, New Delhi, 1957. 442 pp. $5.

Mathematical Excursions. Side trips along paths not generally traveled in elementary courses in mathematics. Helen Abbott Merrill. Dover, New York, 1957 (unabridged and unaltered republication of ed. 1). 156 pp. Paper, $1.

The Golden Bough. A study in magic and religion. Sir James George Frazer. Macmillan, New York, 1958 (reissue of one-volume abridged edition). $3.95.

Scientific Societies in the United States. Ralph S. Bates. Technology Press and Columbia University Press, New York, ed. 2, 1958. 308 pp. $6.50.

TV and Our School Crisis. Charles A. Siepmann. Dodd, Mead, New York, 1958. 198 pp. $3.50.

Exploring the Distant Stars. Thrilling adventures in our galaxy and beyond. Clyde B. Clason. Putnam's, New York, 1958. 384 pp. $5.

A Treasury of Superstitions. Claudia De Lys. Philosophical Library, New York, 1957. 315 pp. $4.75.

Social Perspectives on Behavior. A reader in social science for social work and related professions. Herman D. Stein and Richard A. Cloward, Eds. Free Press, Glencoe, Ill., 1958. 685 pp. $7.50.

NEW Academic Press Books

BIOLOGICAL ULTRASTRUCTURE

By ARNE ENGSTRÖM, *Karolinska Institutet, Stockholm, Sweden* and J. B. FINEAN, *The Medical School, University of Birmingham, England*

February 1958, 326 pp., illus., $8.00

CONTENTS: From Microscopic Morphology to Molecular Structure. Methods in Ultrastructural Research. The Principles of Molecular Structure. The Role of Proteins. The Role of Lipids. The Role of Carbohydrates. Role of Nucleic Acids. Role of Mineral Salts. The Role of Ultrastructure in Biology and Medicine. AUTHOR INDEX—SUBJECT INDEX.

BIOCHEMICAL CYTOLOGY

By JEAN BRACHET, *Université Libre de Bruxelles, Belgique*

1957, 516 pp., illus., $8.80

CONTENTS: Introduction: The Recent History of Biochemical Cytology. Brief Survey of the Techniques. The Cytoplasm of the Resting Cell. The Nucleus of the Resting Cell. Mitosis. Nucleic Acids in Heredity and Protein Synthesis. Nucleocytoplasmic Interactions in Unicellular Organisms. The Nucleus and the Cytoplasm in Embryonic Differentiation. Remarks on Cancer Cells. Final Remarks. SUBJECT INDEX.

INTRODUCTION TO ENZYMOLOGY

By ALAN H. MEHLER, *National Institutes of Health, Bethesda, Maryland*

1957, 425 pp., illus., $10.80

CONTENTS: Introduction. Hydrolysis of Peptides and Proteins. Fermentation and Oxidation of Major Metabolic Fuels. Biological Oxidation: Transfer of Oxygen, Hydrogen, and Electrons. Sugars and Sugar Derivatives. Polynucleotides and Their Components. Amino Acids. Acids and Acid Derivatives. Organization of Structure and Function. SUBJECT INDEX.

QUANTUM MECHANICS OF ONE- AND TWO-ELECTRON ATOMS

By HANS A. BETHE and EDWIN E. SALPETER, *Cornell University*

(Reprinted from the Encyclopedia of Physics, Volume 35)

February 1958, 369 pp., illus., $10.00

CONTENTS: Introduction. The Hydrogen Atom Without External Fields. The Helium Atom without External Fields. Atoms in External Fields. Interaction with Radiation. Appendix on Spherical Harmonics. Bibliography. Addenda and Errata. AUTHOR INDEX—SUBJECT INDEX. INDEX OF TABLES.

CHOLESTEROL

Chemistry, Biochemistry, and Pathology

Edited by ROBERT P. COOK, *Queen's College, University of St. Andrews, Dundee, Scotland*

June 1958, about 530 pp., illus., approx. $15.00

Contributions by D. Adlersberg, W. Bergmann, P. Bladon, G. H. Bourne, G. S. Boyd, R. P. Cook, H. Dam, R. G. Gould, O. Hechter, Marjorie G. Horning, M. F. Oliver, I. H. Page, J. B. M. Rattray, H. Sobotka, and Thressa C. Stadtman.

BIOPHYSICAL CHEMISTRY

By JOHN T. EDSALL, *Harvard University* and JEFFRIES WYMAN, *UNESCO, Cairo, Egypt*

Volume 1:

Thermodynamics, Electrostatics, and the Biological Significance of the Properties of Matter

January 1958, 699 pp., illus., $14.00

CONTENTS: Biochemistry and Geochemistry. Water and Its Biological Significance. Problems of Protein Structure. Thermodynamics. Electrostatics: Its Application to Polar Molecules and Ionic Solutions. Dielectric Constants and Their Significance. Conductivity of Electrolytes. Acid-Base Equilibria. Polybasic Acids, Bases, and Ampholytes, Including Proteins. Carbon Dioxide and Carbonic Acid. Some General Aspects of Molecular Interactions. AUTHOR INDEX—SUBJECT INDEX.

Volume 2, in preparation:

Physical Chemistry of Macromolecules and of Blood

QUANTUM CHEMISTRY

An Introduction

By WALTER KAUZMANN, *Princeton University*

1957, 744 pp., illus., $12.00

CONTENTS: Introduction. Mathematical Background. General Principles of Quantum Mechanics. Atomic Systems. Molecular Systems. Systems in Non-Stationary States. Appendixes. SUBJECT INDEX.

REVIEW: "The mathematical introduction, written at a level well within the grasp of most chemists, is outstanding. . . . The reviewer ventures to predict that the text will be widely adopted."

—*Journal of the American Chemical Society*

BIOCHEMISTRY OF THE AMINO ACIDS

By ALTON MEISTER, *Tufts University School of Medicine, Boston, Massachusetts*

1957, 485 pp., illus., $10.00

CONTENTS: The Natural Amino Acids. The Role of Amino Acids in Nutrition. General Biochemical and Physiological Considerations. Intermediary Metabolism of the Amino Acids. Abnormalities of Amino Acid Metabolism in Certain Pathological Conditions. AUTHOR INDEX—SUBJECT INDEX.

Descriptive leaflets available upon request

ACADEMIC PRESS INC., Publishers

111 Fifth Avenue, New York 3, New York

The Taylor Series. An introduction to the theory of functions of a complex variable. P. Dienes. Dover, New York, 1957 (unabridged and unaltered ed. 1 with errata incorporated in the text). 562 pp. Paper, $2.75.

Safety Techniques for Radioactive Tracers. J. C. Boursnell. Cambridge University Press, New York, 1958. $1.75.

Radioisotopes. A new tool for industry. Sidney Jefferson. Philosophical Library, New York, 1958. 118 pp. $4.75.

The Philosophy of Humanism. Corliss Lamont. Philosophical Library, New York, ed. 4, 1958. 254 pp. $2.75.

Methods in Medical Research. vol. 7. James V. Warren, Ed. Year Book, Chicago. 237 pp. $7.50.

Index of Books Reviewed in SCIENCE, 26 April 1957 through 18 April 1958

Astronomy

Astronomical Optics and Related Subjects, Z. Kopal, Ed. (North-Holland; Interscience), 29 Nov. 1957, 1122

Galactic Nebulae and Interstellar Matter, J. Dufay (Philosophical Library), 20 Sept. 1957, 565

The Galactic Novae, C. Payne-Gaposchkin (North-Holland; Interscience), 27 Dec. 1957, 1350

The Making of a Moon, A. C. Clarke (Harper), 22 Nov. 1957, 1070

Man Among the Stars, W. D. Miller (Criterion), 29 Nov. 1957, 1120

The Milky Way, B. J. Bok and P. F. Bok, ed. 3 (Harvard Univ. Press), 25 Apr. 1957, 829

Morphological Astronomy, F. Zwicky (Springer), 14 Feb. 1958, 343

Radioastronomie, R. Coutrez (Observatoire Royal de Belgique), 17 Jan. 1958, 153

The Stars Above Us, E. Zinner (Scribner's), 31 Jan. 1958, 283

The Sun, G. Abetti (Macmillan), 25 Apr. 1957, 828

Biochemistry and Microbiology

Advances in Enzymology and Related Subjects of Biochemistry, vol. 18, F. F. Nord, Ed. (Interscience), 15 Nov. 1957, 1023

Biochemical Individuality, R. J. Williams (Wiley; Chapman & Hall), 10 May 1957, 940

Biochemical Problems of Lipids, G. Popják and E. Le Breton, Eds. (Interscience), 13 Sept. 1957, 515

Biochemistry and Human Metabolism, B. S. Walker, W. C. Boyd, I. Asimov (Williams & Wilkins), 21 Mar. 1958, 641

The Biochemistry of Vitamin B_{12}, R. T. Williams, Ed. (Cambridge Univ. Press), 31 May 1957, 1094

Bioenergetics, A. Szent-Györgyi (Academic Press), 27 Sept. 1957, 615

Colorimetric Analysis, vol. I, N. L. Allport and J. W. Keyser (Chapman & Hall), 10 Jan. 1958, 89

Dictionary of Microbiology, M. B. Jacobs, M. J. Geestein, W. G. Walter (Van Nostrand), 25 Oct. 1957, 847

The Life of Bacteria, K. V. Thimann (Macmillan), 6 Sept. 1957, 455

Methods of Biochemical Analysis, vol. 4, D. Glick, Ed. (Interscience), 30 Aug. 1957, 407

Methods in Enzymology, vol. 3, S. P. Colowick and N. O. Kaplan (Academic Press), 21 June 1957, 1251

Plant Virus Serology, R. E. F. Matthews (Cambridge Univ. Press), 27 Sept. 1957, 616

Recent Progress in Hormone Research, vol. 13, G. Pincus, Ed. (Academic Press), 14 Feb. 1958, 344

Some Principles of Energetics in Biochemical Reactions, I. M. Klotz (Academic Press), 23 Aug. 1957, 364

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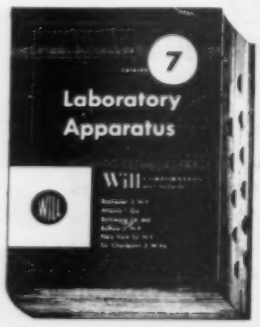

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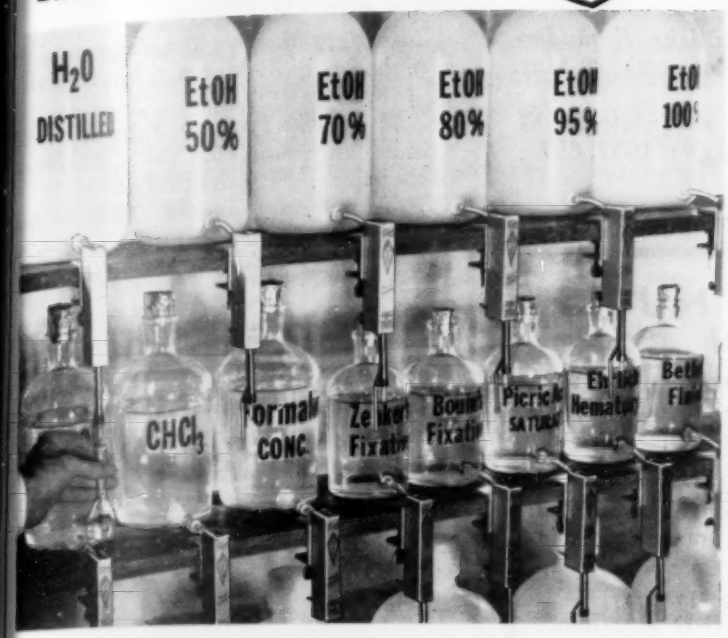

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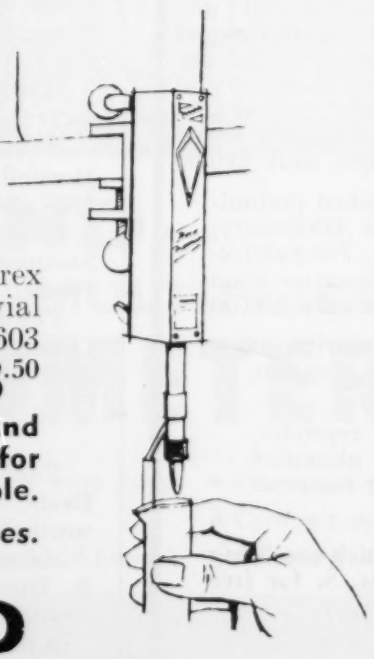

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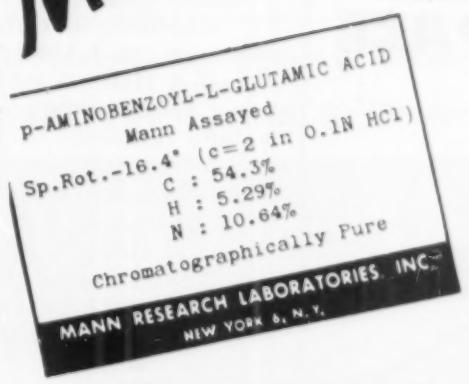

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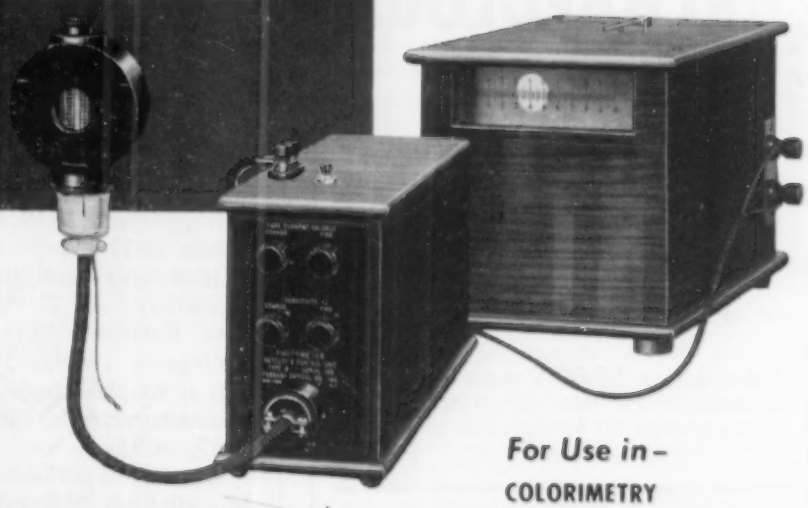

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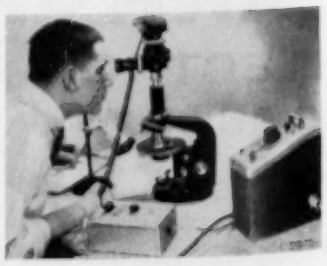

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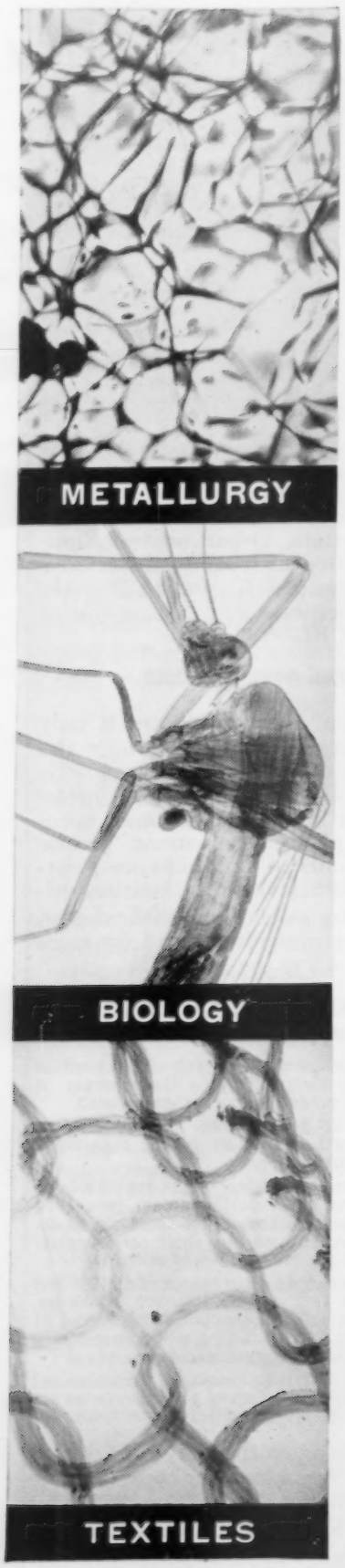

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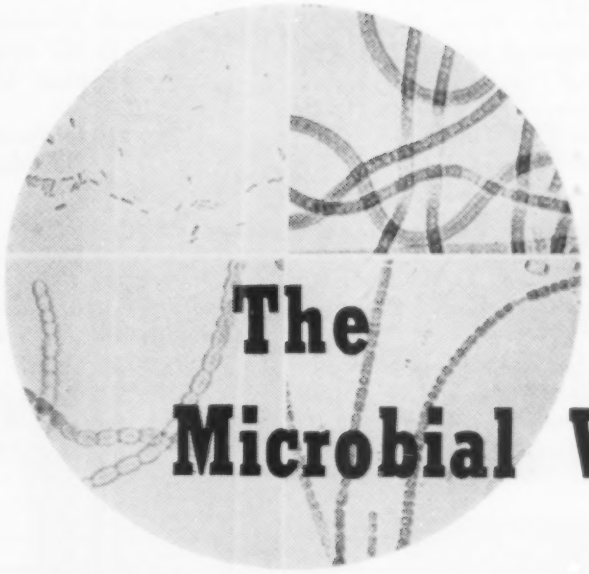

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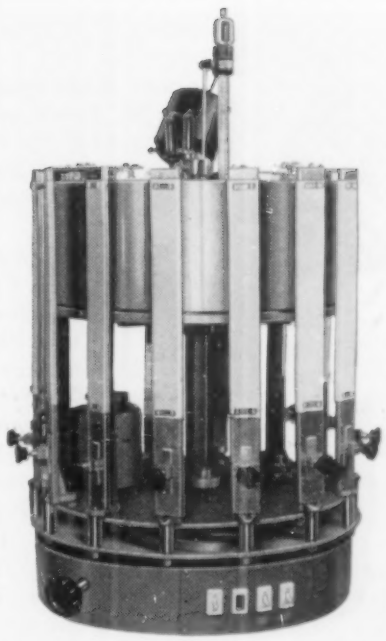

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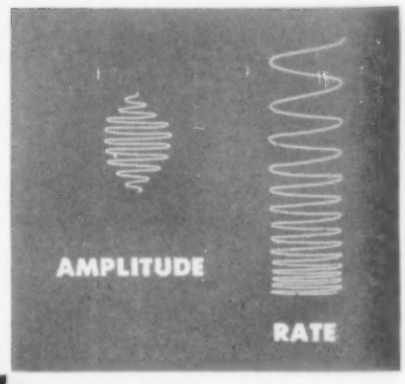

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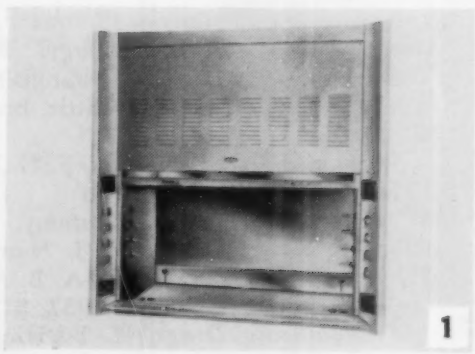
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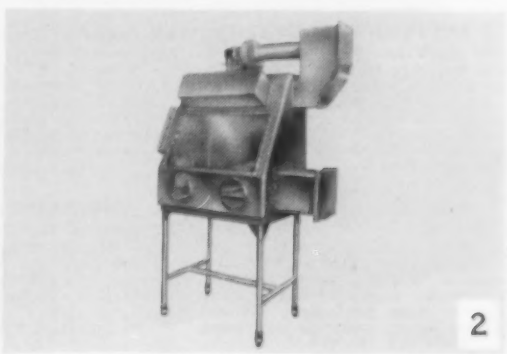
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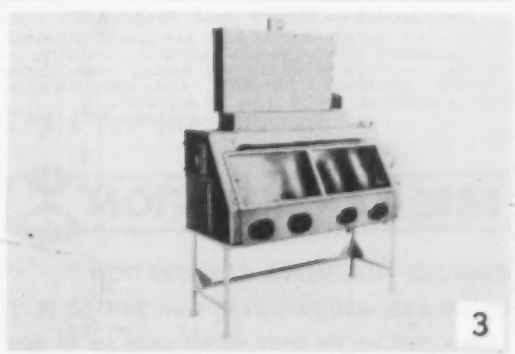
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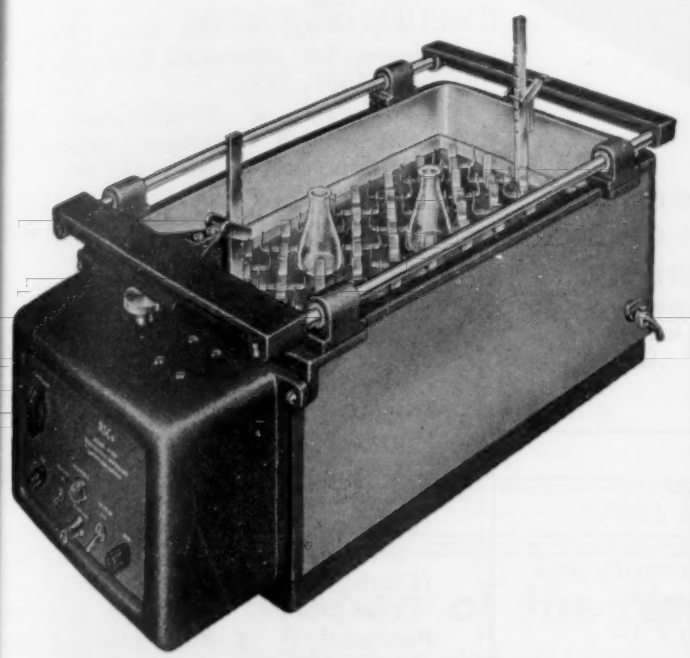

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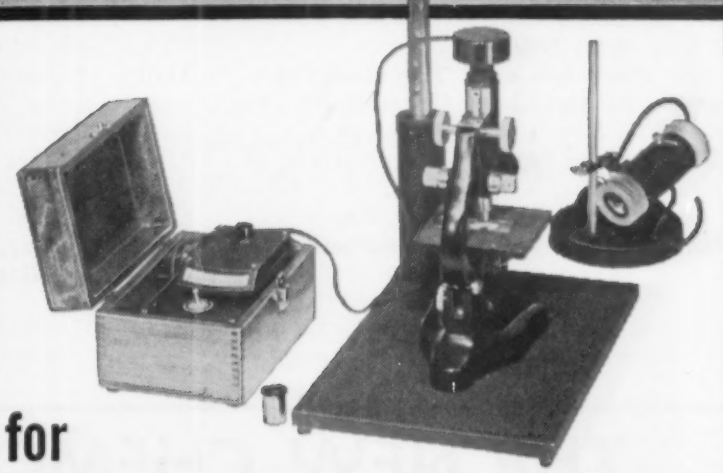

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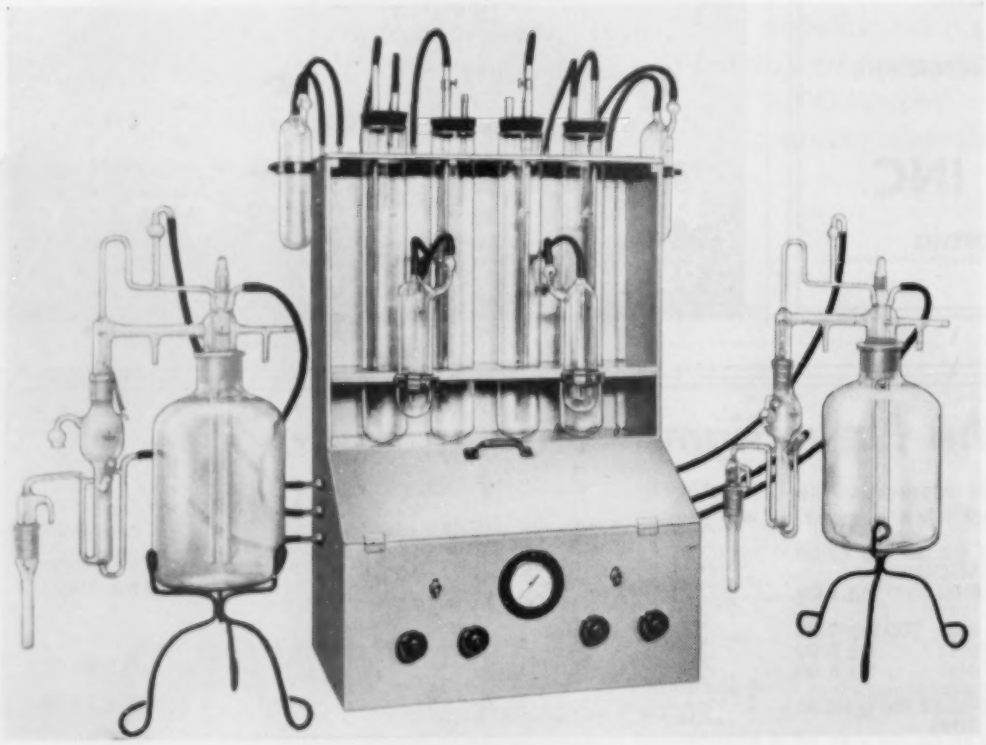

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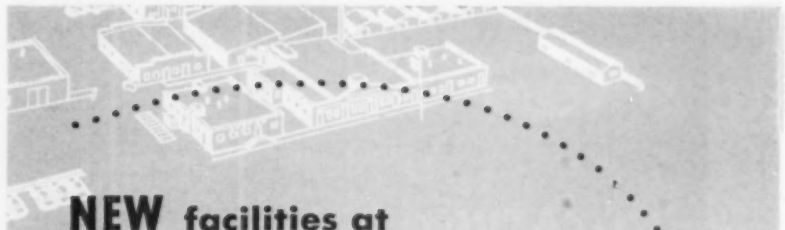

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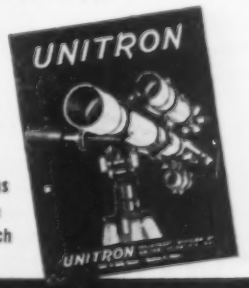

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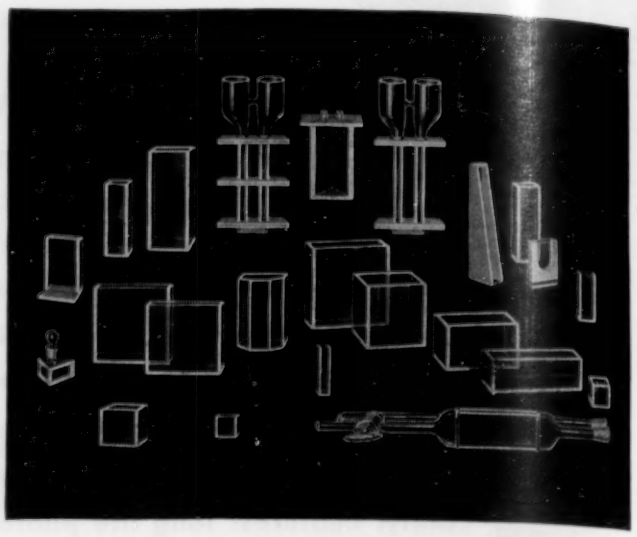

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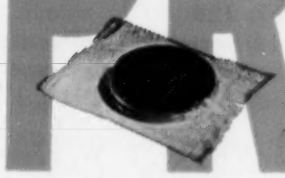

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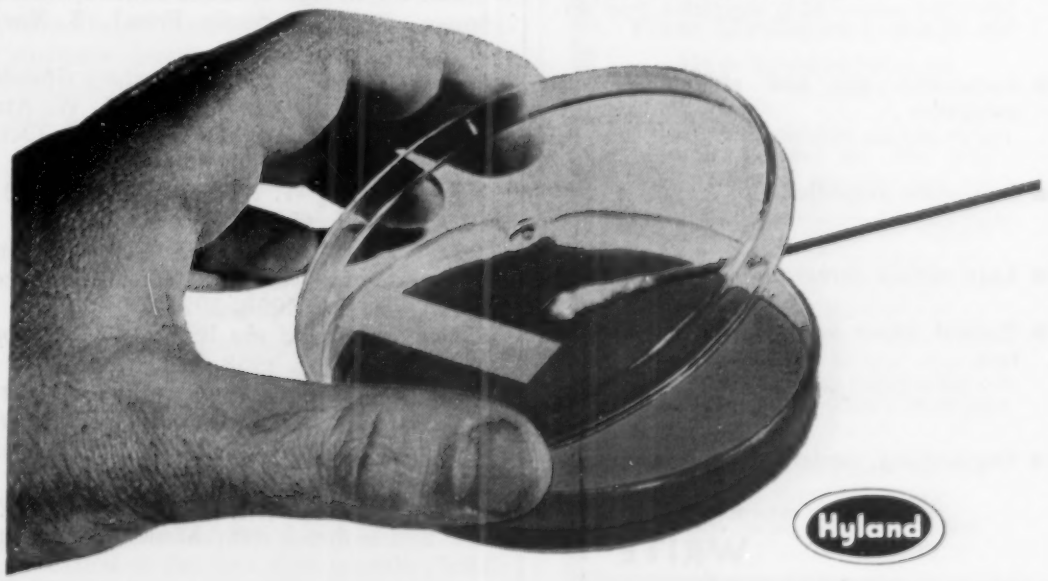

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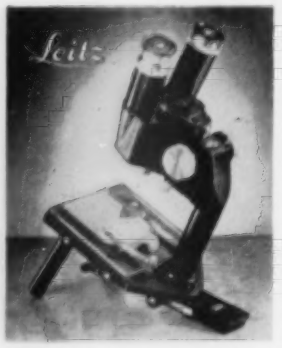

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Letters

On the Pay of Biologists

The announcement "Pay Up for Some Government Scientists" in a recent issue of *Science* [**127**, 21 (3 Jan. 1958)] states that biologists are not included in the pay increase. This seems to be an old and perennial problem among the biologists. I am reminded of the following note I came across this past summer while reading through some old correspondence of the naturalists (Hyatt, Packard, Putnam, and Morse) who founded the Peabody Museum of Salem, the *American Naturalist,* and the American Society of Naturalists.

Rochester, New York
May 18, 1870

I am well aware that works of scientific character are not remunerative, and regret that you have not received the money [subscription money sent to the *American Naturalist,* not properly credited]. Scientific men get much less for their services than any other class of workers. Audubon's great work on birds was a failure in a pecuniary point of view. Le Baliant, the great African traveler, died absolutely poor. I am informed our great American botanist Asa Gray gets the meager sum of fifteen hundred dollars a year for his valuable services. And many others might be mentioned of like character.

Sincerely yours,
ROBERT BUNKER

The biologist continues to be low man on the scientific totem pole.

RALPH W. DEXTER
Department of Biology, Kent State University, Kent, Ohio

Department of Science

In the editorial of the 31 January issue of *Science* [**127**, 213 (1958)], you question the effectiveness and usefulness of a Secretary of Science, and it seems to me that you prefer the idea of adding a science adviser to some Government departments. There may be a possibility of joining these two ideas: The Secretary of Science would appoint the science advisers for the different departments, so that no overlapping would occur. In this way the Secretary of Science would have a great responsibility and a very useful task.

But this is not all. There is no longer any value in having a combined Department of Health, Education, and Welfare. Each activity in itself is too great a task for the three to be handled together by one department. The best thing would be to separate the "Education" and to put it under the Secretary of Science. There are so many things to decide, to order, to explore, and to alter that the Department of Science would be fully occupied. There would be one office which could make decisions for the nation and for all levels of schools as well

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as for all types of schools. The curriculum of a school should no longer be determined by a local school-board or by school boards in certain states; this is a national matter, and there must be nationwide direction. All these supervisory activities must be handled uniformly, in one central place.

ROBERT LOBSTEIN
Santa Monica, California

Actinomycetes

There has been, in recent years, a most unfortunate tendency to publish names of presumably new species of microorganisms, notably actinomycetes, without accompanying such names by proper descriptions. This tendency has been greatly aggravated by the growing interest in describing or in patenting new antibiotics, where a new name for an organism producing such an antibiotic might carry additional weight for priority for independent discovery, or for patentability. Enough problems are raised when names of organisms are published with insufficient descriptions; these difficulties are compounded when no descriptions are given at all. Often a new name is announced, accompanied by one of the following statements:

"This organism appears to be different from those reported in the literature."

"This organism proved to be a hitherto undescribed actinomycete and is named ———."

"Since the characteristics of this organism were not in accordance with those of any of the *Streptomyces* listed in Bergey's *Manual of Determinative Bacteriology,* it was given the name ———."

Sometimes no evidence is presented that a thorough search of the literature has been made, and frequently the new name is not even accompanied by a rudimentary description.

This practice of creating new species, not accompanied by valid descriptions, is to be thoroughly condemned. It violates both the botanical and bacterial codes of nomenclature. It is thoroughly unscientific.

I would like to recommend, therefore, that journals refrain from accepting papers for publication in which new species are listed without accompanying descriptions of the organisms or without references to prior publications of such descriptions.

Incidentally, while I am on the subject of scientific usage, attention should be called to the fact that many scientific journals have adopted the practice of avoiding the use of trade names for antibiotics in scientific papers but insist upon the proper use of scientific names in such papers. Thus, the name "chloramphenicol" is used in scientific litera-

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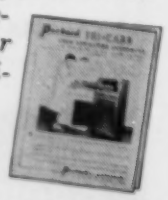

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RALPH A. LEWIN
Marine Biological Laboratory,
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Science and Religion

In his recent article, "Science and the citizen" [*Science* **126**, 1225 (1957)], Warren Weaver makes the following statement: "I would suggest that an absolutely critical distinction between science and religion may be that science never will and never can actually reach the final goal of perfection, whereas religion can do so and has done so." No true scientist will disagree with the statement that science can never achieve perfection. The essence of science is the capacity for objective self-criticism and the realization that today's concepts must constantly be revised in the light of new knowledge.

But has religion achieved perfection? Assuming that the morals and ethics of human society are related to the practice of religion, can we claim "perfection" in this area? If so, why are we so concerned today about crime, social injustice, and world peace?

Religion, like science and all other human activities, undergoes change—it evolves. Julian Huxley, in *Man in the Modern World* (1948), traces the evolution of religion from primitive man's attempts to explain and abate the forces of nature, through polytheism, to monotheism. He concludes that the ultimate stage in this evolution will be a religion that is largely "a deep concern for the welfare of one's fellow man with God absent or merely in the background." Whether we like it or not, the principal religions of the world are steadily moving toward liberalism and humanism, and away from fundamentalism. For example, a survey of the religious beliefs of prominent scientists in the United States [H. J. Leuba, *The Belief in God and Immortality* (1916)] showed that only 21 percent believed in a personal God—that is, a God capable of interceding in the affairs of man.

How can anything that is changing and evolving be considered to be "perfected"? Is any human activity "perfect"? But suppose we concede that at some time and place in the world there was (or is) a "perfect" religion. Where does this concept lead? It means that the "perfect" object must be defended against all change, because any change is bad—it is away from perfection. We force ourselves into a position of defending the status quo at all costs. Further investigation, discussion, or criticism must be prevented because they lead not to improvement but to "heresy." This is the doctrine of infallibility based on authority. To such a model of "perfection" science is a real threat. The methods of science depend on the light of unbiased new truth, a devotion to self-criticism, and a capacity for change.

As Dr. Weaver points out, the layman is often concerned about the so-called conflict between science and religion. Einstein, in his essay on "Science and Religion" [*Out of My Later Years* (1941)], discusses the source of this historical conflict. It occurs chiefly in the area of interpretation of natural phenomena. According to Einstein, religion is incapable of establishing the cause-and-effect relationships of physical occurrences in nature, but its insistence on doing so has led to innumerable conflicts in the past, most notably the clashes that arose with the discoveries of Copernicus, Galileo, and Darwin. He further states that "the doctrine of a personal God interfering with natural events could never be refuted, in a real sense, by science, for this doctrine can always take refuge in those domains in which scientific knowledge has not yet been able to set foot. A doctrine which is able to maintain itself not in clear light but only in the dark, will of necessity lose its effect on mankind with incalculable harm to human progress."

How does this doctrine of "perfection" in religion affect the layman's understanding of science and religion? It requires the conviction that, in any conflict between science and religion, religion is right and science is wrong. It undermines confidence in the objective methods of science and in the validity of its accomplishments. The layman is apt to regard lack of agreement among scientists as a sign of weakness rather than as a source of strength. But, most of all, the layman is likely to mistake the enforced conformity of authoritarian religions for evidence of Truth.

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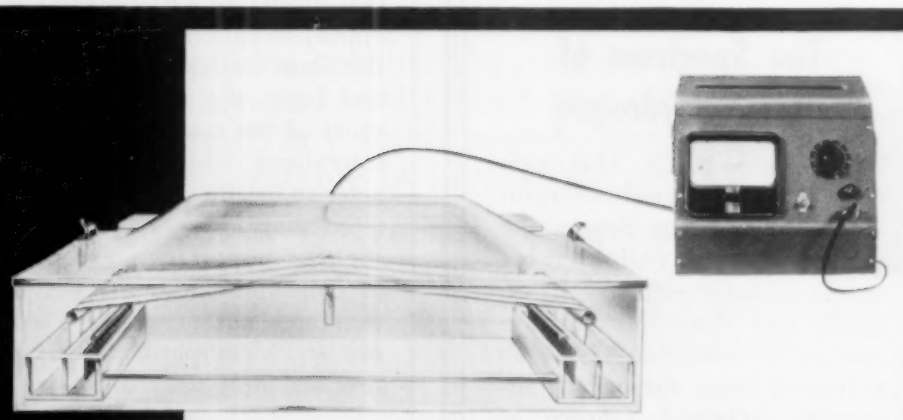

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CLAUDE H. HILLS
Flourtown, Pennsylvania

Warren Weaver's valuable article "Science and the citizen" [*Science* **126**, 1225 (1957)] was unfortunately marred, it seems to me, by the desire to show some deference to religion. The belief of the "average citizen" that "science has destroyed the element of faith in religion" is surely closer to the mark than the assertion that "science is itself founded on faith." In this context, the term *faith* merely begs the question. What is this "faith" on which science is founded? I would suggest that it takes two forms: in experimental science, the appeal to experiment and observation as the court of last resort; in mathematics and logic, the appeal to reason as the court of last resort. Certain esthetic considerations, which are difficult to characterize briefly, also exert their influence.

Neither form of "faith" attaches the slightest importance, except as a matter of convenience, to the weight of tradition and authority. As a consequence, neither is compatible with any form of revealed religion.

ERWIN KLINGSBERG
Mountainside, New Jersey

I am most gratified that Claude H. Hills, Erwin Klingsberg, and several other correspondents have been interested in the highly condensed (and thus necessarily unsatisfactory) references which I made to religion in the paper "Science and the citizen." In response to these comments I have been writing out a fuller statement of my views on the points involved. Unfortunately I cannot meet the requirement of the editor of *Science* that the answer to a question contain no more words than the question does. My reply will be published before long in another journal, and perhaps, when this occurs, the editor will insert a brief notice and reference.

WARREN WEAVER
New York, New York

Meetings

American Astronautical Society

The American Astronautical Society, founded in 1953 and incorporated in New York State in 1954, is a scientific organization dedicated to advancement of the astronautical sciences. The society considers manned interplanetary space flight a logical progression from today's high-performance research plane, guided missile, and earth satellite operations. The scope of the society is indicated by a partial list of the astronautic fields of interest: astronavigation, biochemistry, celestial mechanics, cosmology, geophysics, and space medicine, as well as space vehicle design, including communications, control, guidance, and propulsion.

Promotion of astronautics is accomplished by the society largely through its program of technical meetings and publications. The fourth annual meeting, held in New York City 29 to 31 January, was attended by over 600 members and guests. Forty-five original papers were presented, in technical sessions on "Space vehicle design," "Space exploration," "Guidance and control," "Man's environment in space," "Space vehicle communications," and "Astronautics research." At the Honors Night dinner presentations were made to recipients of the AAS Space Flight award, the Melbourne W. Boynton award for space medicine, and the Annual Fellow awards. Plans are currently being formulated to hold the fifth annual meeting in Washington, D.C., at the end of December, in conjunction with the annual meeting of the AAAS. In addition, a West Coast regional AAS meeting will be presented in San Francisco in August.

Regional sections of the American Astronautical Society have been formed in New York, San Francisco, and Washington, D.C. Technical meetings, dinners, and field trips are among the activities arranged by these sections. Considerable interest has been expressed by AAS members and others in the formation of new regional sections in Los Angeles, Baltimore, Dallas, Philadelphia, Dayton, Boston, and Chicago. It is expected that most of these groups will be chartered as AAS regional sections during 1958.

Publications of the society include the *Journal of the Astronautical Sciences*, *Proceedings* of the AAS annual meetings, and reprints of technical papers. At the present time the *Journal* is published quarterly and incorporates the "Astronautical Sciences Review." Ultimately, it is planned to publish the "Astronautical Sciences Review" separately as a companion magazine which would contain articles of general interest, AAS news, abstracts, book reviews, and so on.

Membership in the society is com-

posed primarily of scientists and engineers, although all persons having a sincere interest in astronautics, including students, are eligible for special grades of membership. Each year the board of directors and the fellows of the society elect as fellows those who have made direct and significant contributions to the astronautical sciences. At the present time, the roster of AAS members includes about 600 persons. Additional support of the society is sought from American industry. There are presently seven AAS corporate members: Varo Manufacturing Company; Avion Division–ACF Industries, Inc.; Douglas Aircraft Company; Republic Aviation Corporation; Space Corporation; Kearfott Company, Inc.; and the Martin Company.

The society cooperates fully with other national and international scientific organizations; it is a member of the International Astronautical Federation and an affiliate of the American Association for the Advancement of Science. Paul A. Campbell of the U.S. Air Force Office of Scientific Research has been appointed the 1958 representative on the AAAS council.

ROSS FLEISIG
Sperry Gyroscope Company,
Great Neck, New York

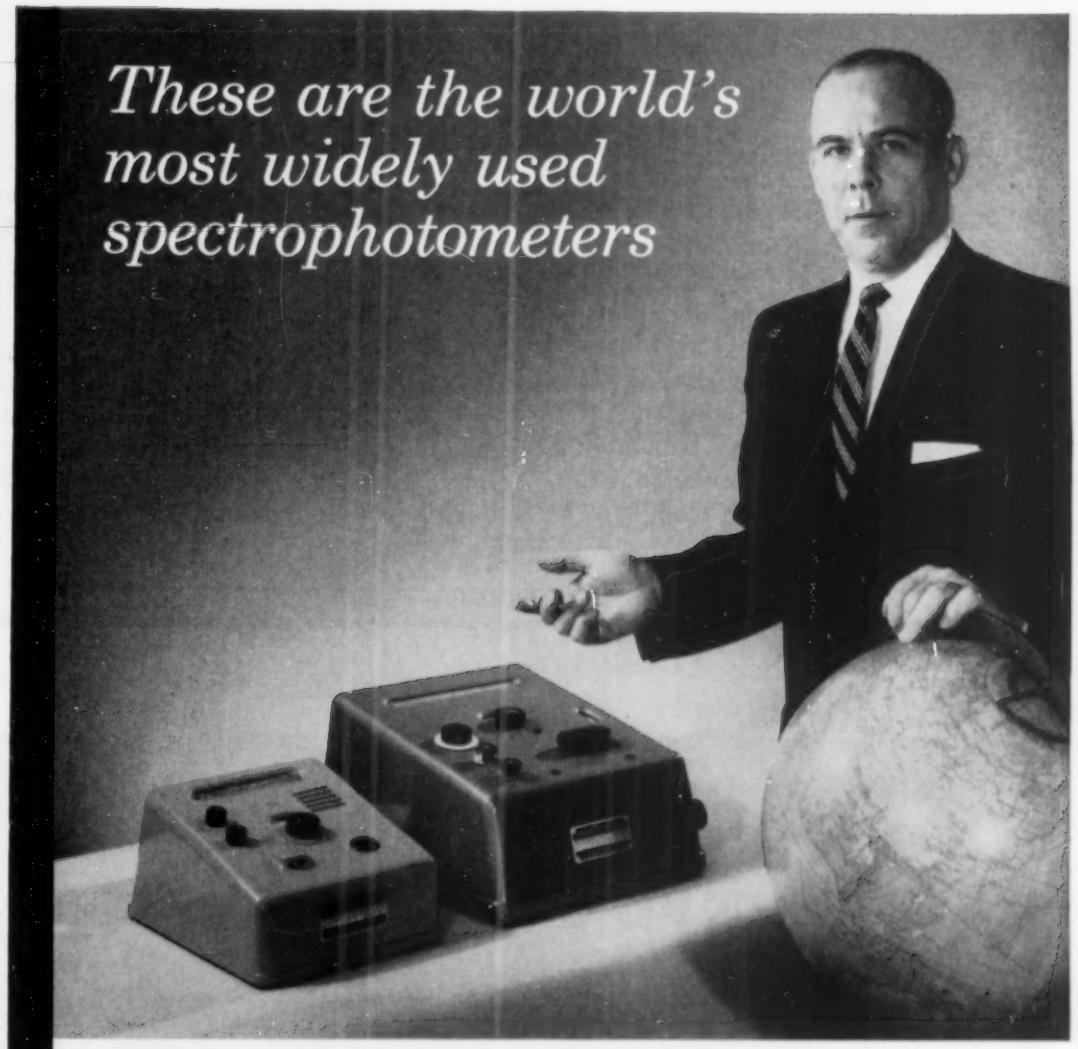
Chemical Organization of Cells

A Conference on the Chemical Organization of Cells, Normal and Abnormal, will be held in Madison, Wis., 21, 22, and 23 August. Participants will be specialists in anatomy, biochemistry, embryology, and pathology who will provide correlated recent information about the cell in a series of formal presentations. Inquiries should be addressed to: Dr. Joseph J. Lalich, Professor of Pathology, University of Wisconsin, Madison 6, Wis.

A limited number of travel stipends may become available later this spring. Information will be advertised in appropriate journals and circularized to those who have already registered with Dr. Lalich for the conference.

Physiological Sciences

The 21st International Congress of Physiological Sciences (physiology and pharmacology) will be held in Buenos Aires, Argentina, 9–15 August 1959. Bernardo A. Houssay will be the president. The Organizing Committee consists of E. Braun Menéndez, chairman; A. O. M. Stoppani, secretary; E. Strajman, treasurer; and M. H. Burgos, H. Chiodi, T. Combes, E. D. P. De Robertis, J. C. Fasciolo, V. G. Foglia, E. Hug, A. Lanari, L. F. Leloir, J. T. Lewis, R. E. Mancini, E. Moisset de Espanes, E. L. Rabasa, L. M. Rinaldini,



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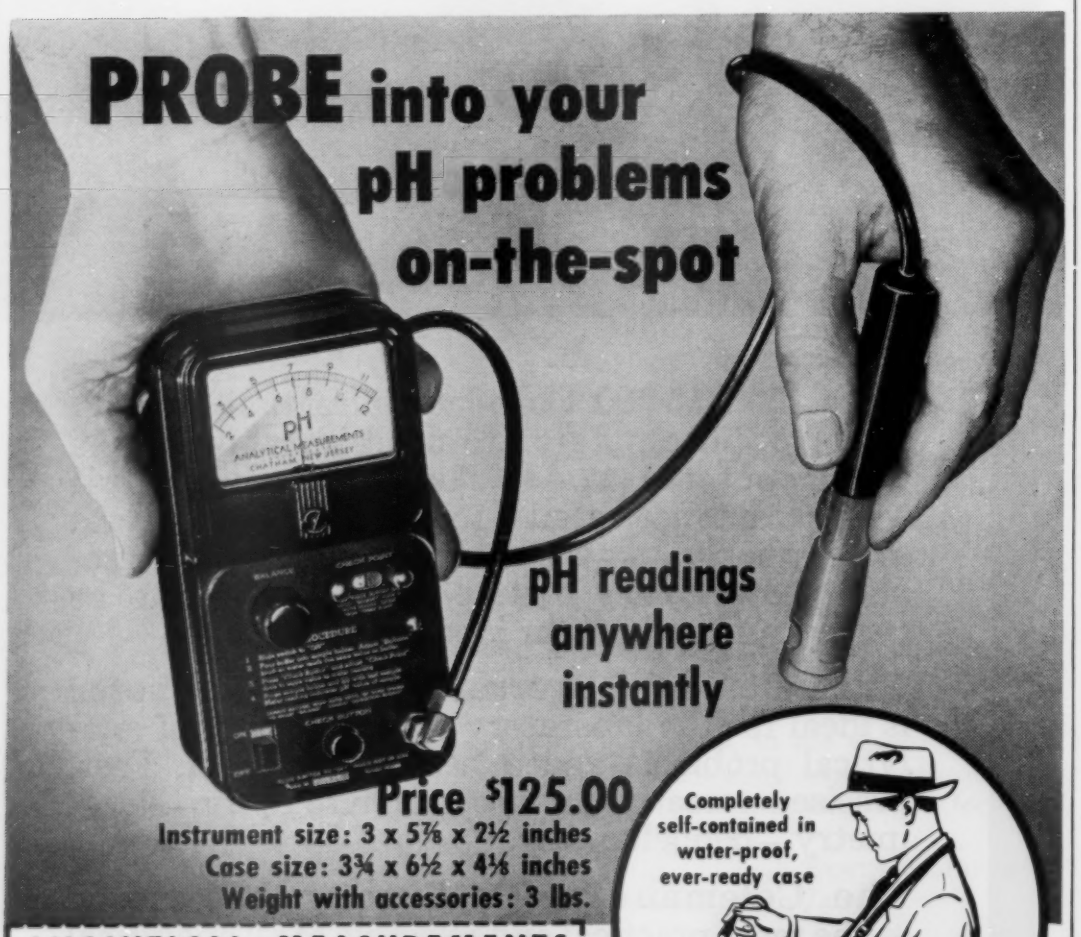
R. R. Rodriguez, J. R. Suarez, S. Taleisnik, C. A. Taquini, and M. E. Varela.

This congress is the first of its kind to be held in the Southern Hemisphere and, to make it possible, many international institutions are lending their financial support. Contributions have been received from private organizations such as Burroughs Wellcome & Co. Inc., Ciba Company, Eli Lilly and Company, Hoffmann-La Roche Inc., Merck & Co., Merrell National (Overseas) Laboratories, Miles Laboratories, Inc., Olin Mathieson International Corporation, and the Upjohn Company.

Sensory Deprivation

A Symposium on Sensory Deprivation will take place in Boston, Mass., 20–21 June, under the joint auspices of the Psychiatric Research Laboratory, Boston City Hospital, Harvard University Medical School, and the Office of Naval Research (Physiological Psychology Section). The symposium will be a closed working meeting of invited speakers and discussants. Proceedings will be published. The planning committee consists of Philip Kubzansky, P. Herbert Leiderman, Jack H. Mendelson, Donald Wexler, and Philip Solomon (chairman).



Society Elections

■ Econometric Society: pres., James Tobin, Department of Economics, Yale University; v. pres., M. Marcel Boiteux, Paris, France; sec., Richard Ruggles, Department of Economics, Yale University, New Haven, Conn.; treas., Nancy Ruggles, New Haven, Conn. The representative to the AAAS Council is Charles F. Roos, New York, N.Y.

■ Western Society of Naturalists: pres., and representatives to the AAAS Council, Tracy I. Storer, University of California at Davis; past-pres., and representative to the AAAS Council, William M. Hiesey, Carnegie Institution of Washington, Stanford, Calif.; v. pres., Herbert L. Mason, Department of Botany, University of California, Berkeley; treas., Robert L. Fernald, Department of Zoology, University of Washington; sec., John P. Harville, Department of Biology, San Jose State College, San Jose 14, Calif.

■ Ecological Society of America: pres., Stanley A. Cain, School of Natural Resources, University of Michigan; v. pres., Thomas Park, Hull Zoological Laboratory, University of Chicago; sec., John E. Cantlon, Department of Botany and Plant Pathology, Michigan State University, East Lansing, Mich.; treas., Jack S. Dendy, Department of Zoology and Entomology, Alabama Polytechnic Institute. The representatives to the AAAS Council are Henry J. Oosting and W. Frank Blair.

■ Florida Academy of Sciences: pres., Dan A. Thomas, Physics Department, Rollins College; pres.-elect, and representative to the AAAS Council, E. Ruffin Jones, Department of Biology, University of Florida; sec., Guenter Schwarz, Department of Physics, Florida State University, Tallahassee, Fla.; treas., Alex G. Smith, Department of Physics, University of Florida.

Forthcoming Events

May

25–29. Air Pollution Control Assoc., 51st annual, Philadelphia, Pa. (H. M. Pier, APCA, 4400 Fifth Ave., Pittsburgh.)

25–29. Institute of Food Technologists, annual, Chicago, Ill. (C. S. Lawrence, IFT, 176 W. Adams St., Chicago 3.)

25–31. International Soc. of Gastroenterology, 3rd world cong., Washington, D.C. (H. M. Pollard, University Hospital, Ann Arbor, Mich.)

26–28. American Soc. for Quality Control, annual, Boston, Mass. (W. P. Youngclaus, Jr., ASQC, 161 W. Wisconsin Ave., Milwaukee 3, Wis.)

26–29. Comparative Endocrinology Symp., Cold Spring Harbor, N.Y. (Symposium of Comparative Endocrinology, Dept. of Zoology, Columbia Univ., New York 27.)

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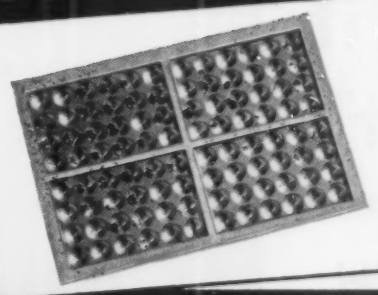

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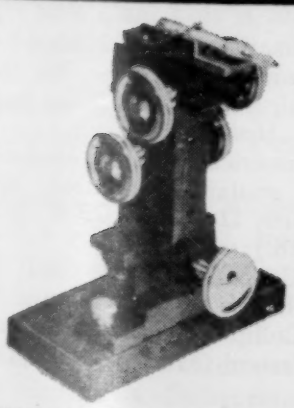

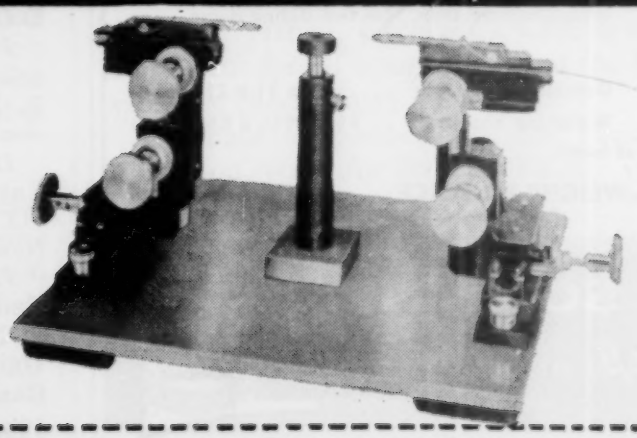

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27–31. Thermal and Hydraulic Power Stations, Liége, Belgium. (A. Biron, 1, rue de Spa, Liége.)

28–8. European Federation of Chemical Engineering, 2nd cong., Brussels, Belgium and Frankfurt/Main, Germany. (Deutsche Gesellschaft für Chemisches Apparatewesen, Rheingau-Allee 25, Frankfurt/Main.)

29–31. American Acad. of Dental Medicine, 12th annual, Montreal, Canada. (G. Witkin, AADM, 45 S. Broadway, Yonkers 2, N.Y.)

31–8. European Federation of Corrosion, 2nd cong., Frankfurt/Main, Germany. (Gesellschaft Deutscher Chemiker, Haus der Chemie, Karlstrasse 21, Frankfurt/Main.)

June

2–4. Telemetering Conf., 6th natl., Baltimore, Md. (G. M. Thynell, Applied Physics Lab., Johns Hopkins Univ., Silver Spring, Md.)

2–5. American Nuclear Soc., 4th annual, Los Angeles, Calif. (ANS, P.O. Box 963, Oak Ridge, Tenn.)

2–6. Mass Spectrometry, 6th meeting, New Orleans, La. (R. A. Friedel, U.S. Bureau of Mines, 4800 Forbes St., Pittsburgh 13, Pa.)

2–6. Medical Library Assoc., 57th annual, Rochester, Minn. (T. E. Keys, Librarian, Mayo Clinic, Rochester.)

2–6. Peaceful Uses of Atomic Energy in Australia, symp., Sydney, N.S.W. (Australian Atomic Energy Commission Research Establishment, Private Mail Bag, Sutherland, New South Wales.)

2–7. Mechanical Engineering, 7th internatl. cong., Scheveningen, Netherlands. (International Mechanical Engineering Cong., 10, avenue Hoche, Paris 8e.)

3–5. Special Libraries Assoc., annual, Chicago, Ill. (M. E. Lucius, SLA, 31 E. 10 St., New York 3.)

4–14. Large Electric Systems, 17th intern. conf., Paris, France. (112, Boulevard Haussmann, Paris.)

5. Institute of Microbiology, 4th annual, New Brunswick, N.J. (E. R. Isaacs, Inst. of Microbiology, Rutgers Univ., New Brunswick.)

9–11. American Assoc. of Spectrographers, 9th annual symp., Chicago, Ill. (H. J. Hettel, Armour Research Foundation, 10 W. 35 St., Chicago 16.)

9–11. Canadian Federation of Biological Societies, 1st annual; with Canadian Assoc. of Anatomists, Canadian Biochemical Soc., Canadian Physiological Soc., and Pharmacological Soc. of Canada, Kingston, Ontario. (E. H. Bensley, Montreal General Hospital, 1650 Cedar Ave., Montreal 25, P.Q.)

9–11. Health Physics Soc., 3rd annual, Berkeley, Calif. (E. E. Anderson, Oak Ridge National Lab., Oak Ridge, Tenn.)

9–11. Soc. of General Physiologists, Woods Hole, Mass. (F. G. Sherman, Dept. of Biology, Brown Univ., Providence 12, R.I.)

9–11. Society for the Study of Development and Growth, 17th annual symp., South Hadley, Mass. (Miss K. Stein, Dept. of Zoology, Mount Holyoke College, South Hadley.)

9–12. Microscopy Symposium, 5th, Chicago, Ill. (W. C. McCrone, Jr., 500 E. 33 St., Chicago 16.)

9–13. Automation Exposition and Cong., 4th Internatl., New York. (International Automation Exposition, c/o Richard Rimbach Assoc., 845 Ridge Ave., Pittsburgh 12, Pa.)

10–12. Astronomical Soc. of the Pacific, annual, Los Angeles, Calif. (S. Einarsson, Leuschner Observatory, Univ. of California, Berkeley 4.)

10–13. Vacuum Techniques, 1st internatl. congress, Namur, Belgium. (E. Thomas, c/o CSN/ERM, 30, avenue de la Renaissance, Brussels 4, Belgium.)

11–14. Applied Mechanics, 3rd natl. Cong., Providence, R.I. (W. Prager, Brown Univ., Providence 12.)

11–14. National Soc. of Professional Engineers, St. Louis, Mo. (P. H. Robbins, NSPE, 2029 K St., NW, Washington, D.C.)

14–21. American Soc. of Medical Technologists, annual, Milwaukee, Wis. (Miss R. Matthaei, Suite 25, Hermann Professional Bldg., Houston 25, Tex.)

15–19. American Soc. of Mechanical Engineers, semiannual, Detroit, Mich. (O. B. Schier, II, ASME, 29 W. 39 St., New York 18.)

15–19. Cancer Research Conf., 3rd Canadian, Honey Harbour, Ontario. (R. L. Noble, Collip Medical Research Lab., Univ. of Western Ontario, London, Ont., Canada)

(*See issue of 18 April for comprehensive list*)

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Equipment

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■ MICROCOMBUSTION APPARATUS, to be used for the determination of halogens, sulfur, phosphate, and so forth in organic materials by the Schoniger technique, consists of a heavy-wall conical flask and a ground-glass stopper to which is attached a platinum sample carrier. The sample is burned within the flask charged with oxygen. (Arthur H. Thomas Co., Dept. 1)

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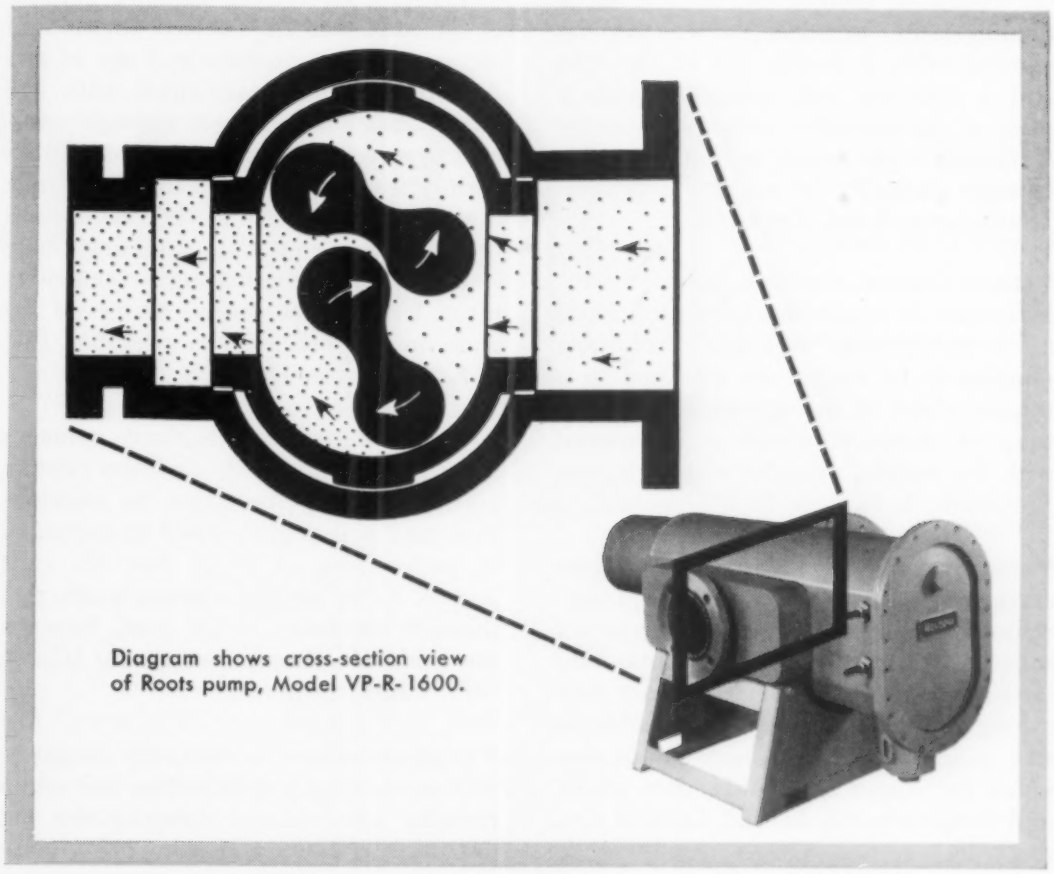

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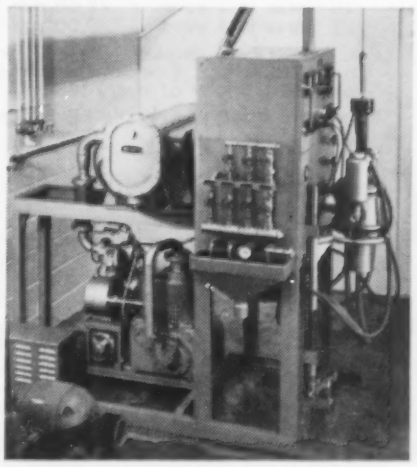

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